A VIEW

OF THE RELATIONS

OF THE

NERVOUS SYSTEM,

In Health and in Disease.

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NERVOUS SYSTEM,

In Health and in Disease;

CONTAINING

SELECTIONS

FROM THE DISSERTATION TO WHICH WAS ADJUDGED
THE JACKSONIAN PRIZE FOR THE YEAR 1813.

WITH ADDITIONAL

Illustrations and Remarks.

BY DANIEL PRING,

MEMBER OF THE ROYAL COLLEGE OF SURGEONS, LONDON;
AND SURGEON AT BATH.

" *Namque alid ex alio clarescet; nec tibi cæca*
" *Nox iter eripiet quin ultima naturäi*
" *Pervideas; ita res adcendent lumina rebus.*"

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ERRATA.

Page	Line	
14 ..	13 ..	for "parellel," read parallel.
219 ..	3 ..	for "effusion," r. affusion.
220 ..	16 ..	for "tendo capillis," r. tendo Achillis.
247 ..	19 ..	for "navi materni," r. nævi materni.

TO

JOHN ABERNETHY, ESQ; F.R.S.

THE FOLLOWING PAGES

Are respectfully inscribed

BY THE AUTHOR.

Bath, May 30, 1815.

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PREFACE.

THERE is, perhaps, no subject in medicine, concerning which so much has been said, so much desired, and so little understood, as that of the diseases of the NERVOUS SYSTEM. We need not inquire with much diligence for the reasons of this dearth of knowledge: it arises from the difficulty of understanding causes, where the senses can inform us only of effects. The same difficulty still continues; and it will both outlive medicine, and every other monument of the art of man.

If theory, which now lies oppressed by accumulated odium, is not altogether shut out from the circle of the physical sciences, it will be likely to find employment among the topics of the following pages. I cannot but suspect that the services of it may here be engaged with advantage; and if this can be made clear, it will serve as an apology for the introduction of a

few sentences which do not profess to describe ocular testimonies.

If theory (by which I mean the assumption of facts by analogies not proved) is allowed to occupy the place of ascertained knowledge; if its pretensions exceed its real claims; if it is made the sanction of practice, and suffered to dictate with confidence where its voice should be silent; why then the odium is just: and a monster which is calculated only to mislead those who would be assiduous in the right path, cannot be too far banished from our acquaintance; it cannot be too peremptorily excluded from the records of our intellectual proficiency.

But to give to theory a rank so much above its legitimate station, is a perversion of the good we are capable of deriving from it; and as we are all apt to believe, notwithstanding the frequency of its condemnation, that this hunting after fact, by supposition, is not entirely without its use; it becomes necessary, in order to employ it with the advantages we assume, to indicate the proper mode of its application.

It is the business of speculation (which differs but little from theory) to precede the de-

monstrative inquiry. This is a natural order: let us therefore not find fault with speculation. Shall we make experiments without an expectation of results? It would be as reasonable to attempt the formation of a code of laws by shaking letters from a bag. Or, having acquired a demonstration, shall we withhold an operation of the mind which enables us to connect it with others; to infer or define all that is proved; to augment the sum of our attainments; to enable us to discriminate between real and apparent; finally, to perfect our practice in the widest range, and to benefit mankind? No; we must not despise these exercises, but we will reject them when they are unfairly conducted.

This, which I have indicated, is the kind of investigation which medicine requires at the hands of its professors. Speculation is the wing of science; its truth is confirmed by experience.

Let us take one example, without any pains about the selection, and see how far the knowledge of unsearched-for results, or experience, according to the common limited sense of the word, unaided by other investigation, will carry us towards the full comprehension of any one subject in medicine. I should begin the exami-

nation of a pupil in this way: I agree with you that this man has phrenitis; the disease is very clearly denoted, and you have drawn a very fair diagnosis. You propose to bleed this man, *because experience has shewn that blood-letting will diminish inflammation:* but such an effect of the remedy is by no means universal, for the disease is often fatal; and further, its course is sometimes in no degree arrested where blood-letting is carried to the greatest possible extent.

From this fact I may presume to ask, whether blood-letting is not irregular in its effects? This question must be answered in the affirmative, for the affirmative is proved by practice. To pursue the order of investigation, I must next demand, what are the laws which govern this irregularity? We have a selection of three effects from blood-letting, which those who observe must have perceived; it may do good, it may be prejudicial, or it may influence only the systems which are already exempt from disease.

With this assurance, it is necessary that we should inspect our subject more closely; that we should discover the government of these anomalies, and be enabled to direct our prescription with truth and advantage. We must in-

spect our subject; we must understand, and that minutely too, anomalous conditions of the body; conditions where latent causes are to be sought for, as well as those which are more conspicuously and familiarly displayed in symptoms. We must not stop here; our *questions* would lead us to inquire into the *general operation* of blood-letting: we must first understand this, and then we might proceed with our exceptions.

Such an understanding would involve all that which should be the basis of our science; but it is at present no better than a great chasm which we have overleaped, and we appear to make light work with the superstructure. The ancients, who were nearer to nature than ourselves, were ambitious to begin with fundamental principles; but it was a hard service, and therefore fell into disuse: it may perhaps at this time be adopted with advantage for the stock of our materials is increased.

We are conducted by these simple considerations to the commencement of an arduous task; we are taught to perceive that there is a vast deal to be *studied:* investigation is set afloat; Reason begins now to extend her operations; she is not contented with the first reply of our

pupil, (viz.) that blood-letting is found to be a good thing, but she begins to work in other directions; she discriminates *genuine analogies* from those which are made spurious by associations; she distinguishes between causes and connexions; she views things at first with obscurity, and she labours to the object with every aid she can collect, dispelling the mists which darken her road; finally, she acquires another ray for mankind from the divine original of truth.

In the following pages, my design has been by no means to exhibit a compendium of the state of the science on any one particular, but merely to ascertain a few facts, and to exhibit occasionally the topics which remain for further investigation. This double view corresponds with the present state of science, and with the means of its improvement. Many labourers are engaged in a progressive work, the end of which is the perfection of the respective departments of knowledge. This design embraces a great scheme of concurrence: it is not the union of a few individuals, or of towns, or of cities, but of nations; and the co-operation is quietly prosecuted through the collisions of politics, the convulsions of states, and is not interrupted even by the infernal spirit of war. The leading

division of employment is to indicate, as by theory, the points which require investigation; and to confirm, as by practice or by experiment, the relations which are suggested by conjecture. The whole business then is comprised in indication, and in confirmation, conformably with the end of research, which is to increase our knowledge, and to exchange supposition for certainty.

With regard to the experiments which are introduced, I pledge myself for the fidelity of the narration. I have mentioned nothing as a fact, of which I have not had all the evidence by which it is recommended. If these facts should be in discord with the pre-conceptions of those whose thoughts have been cast into the same channel, I beg that they will repeat my investigation; and if their results should be different from those which I have mentioned, I shall be happy to be improved by a better instruction.

Although experiment appears to be the only mode of exchanging conjecture for certainty, it is not altogether infallible. We may mistake accidental, for regular, phenomena; we may generalize, only because we are ignorant of exceptions; we may pursue our object with a rigid observance, but circumstances, at the time un-

noticed, may influence results, and detract from the perfect accuracy of our conclusions. Experiments therefore seldom constitute a perfect evidence until they have passed through another ordeal: the truth will perhaps appear from a conjoined examination.

The efforts of intellect are strong and penetrating: let us hope that their continuance will be gratifying to her to whom they are addressed; that Nature, who shuns us when lightly wooed, will be pleased with our zeal for a closer acquaintance; and that, finally, she will be developed to an extent beyond our expectations, in all the beauty which we know her to possess, and which we perceive no less in the obvious character of her works, than by the glimpses which chance, or more strenuous intrusion, has occasionally afforded us.

SECTION I.

NATURAL STRUCTURE, FACULTIES, AND RELATIONS OF NERVES.

ARTICLE I.

Structure of Nerves.

THE examinations which have been directed to the development of the minute structure of the nerves, are such as might be expected from the diligence and scrutiny which have characterized the labours of anatomists, and which are no less conspicuously evinced in this, than in every other department of their science. But these efforts, in the present instance, have not wholly attained the success which they emulated; their object was to explain the function, by the knowledge of the structure, of the nerves. This design has been frustrated by the subtilty of the agents by which their processes are accomplished; and the acquaintance with sensible arrangements has therefore not exposed to us those ultimate relations which are partially displayed in effects, and

but to a very limited extent to be traced to the more palpable contrivances.

By making this limitation, it is not meant to insinuate that the labours of the anatomists have been here altogether destitute of utility: on the contrary, we must confess many obligations for the particulars which their ingenuity has elucidated; and the knowledge which has in this way been conferred, must form a principal ground work of the attempts at a further prosecution of inquiry. For this reason, it is proper to mention these results of anatomical investigation; but it is necessary to extend such notices no farther than will be required by the subsequent considerations, with which they are liable to be connected.

The nerves consist of a membranous tissue, which has been called their neurilema, and of a medullary matter. The substance of the optic nerve is of two kinds, one transparent and the other opaque: the opaque portion consists of a number of cylinders; the transparent is a gelatinous matter, by which these cylinders are connected. The cylindrical bodies, according to Sir E. Home's observations, are the least numerous at the place of connexion between the nerve and the brain, and they increase in number from forty to five hundred, as the nerve

proceeds to the eye. The medulla may be dissolved by means of caustic alkali, and a tubular membrane only remains.

According to the observations of Prof. Reil, the neurilema constitutes the tubuli by which the medulla is enclosed: the connexion of this membranous tissue is not with the central termination of the nerve; it ceases before the nerve joins its centre, and the junction of the former with the latter is then only by means of its medulla. The neurilema is plentifully supplied with blood-vessels: these form a secretory system, by which the medulla is produced, renewed, or variously changed. In this part of the nerve lymphatics also are discovered. The same structure does not obtain in all the nerves: on the contrary, it acknowledges many varieties, which are however no great digression from a general character.

The nerves have been regarded as bundles of tubes; as composed of fibrils with interstitial cells; as composed wholly of fibres; as composed of fibres united by medulla; as globules joined together by gelatinous matter; as fibres united by cellular membrane.

The result of Soemmering's observations with the microscope is, that the structure is not

to be ascertained with the minuteness which has been attempted; and that there are many questions which the closest examination must leave unanswered: as, whether the globules are arranged in right lines? whether they are smaller, as some have affirmed, than those of the blood? whether they are of different sizes? whether they float in a pellucid fluid, or are inclosed in proper capsules? These are questions which have been at different periods discussed; but the ambiguity which must still rest upon such discoveries, although assumed as the results of sensible cognizances, renders them no more to be depended upon than the speculations on the points to which they refer, which might be raised without their assistance.

ARTICLE II.

Retraction of Nerves.

THE fact, that the nerves possess a power of retraction, had been questioned by some, but has been established by Sir E. Home.* His experiments were made on the horse, and the

* Philosophical Transact. of Royal Soc. 1801. (Croonian Lecture.)

retraction, on a simple division, was found to be equal to $\frac{7}{8}$ of an inch. The size of the nerve, and the distance of the retraction, preclude the possibility of a doubtful inference. It was found that the stimulus of electricity could produce no further retraction of a nerve than that which spontaneously succeeded to its division.

I have sometimes had occasion to divide the medulla spinalis, and I have remarked that a retraction took place here also. In one instance, in which the animal lived an hour after the division of the spinal marrow, I found a coagulum interposed between the two extremities of the chord, which were separated by a distance of three lines.* As the retraction of nerves is sufficiently established, I did not measure the extent of it in any of the experiments subsequently detailed, in which the circumstance is adverted to.

It would appear that the retraction of a nerve might take place in two ways, (viz.) either by the contractility of tissue, or as an effect of irritability. That the stimulus of electricity will not increase the spontaneous retraction, inclines us to refer this effect to the contraction which is a property of inanimate matter, and

* This experiment was made on a puppy.

which takes place when the means of extension are discontinued; but this cause of contraction can operate only as long as the extension lasts, and the effect should not take place in a nerve under a state of relaxation.

In order to ascertain whether a retraction does occur under these circumstances, I have approximated the portions of the nerve of a limb by bending a joint, and I have found that the retraction took place at a point where the state of extension was precluded. The retraction therefore of a nerve we may define to be the effect of its irritability; which is further supported by the fact, that the division of a nerve in the dead subject is not succeeded by a retraction. Death is followed by a relaxation of the structures to which nerves are distributed, and the nervous chords participate in this failure of extension; the latter proof, on this account, is perhaps not to be considered conclusive.

ARTICLE III.

Reproduction of Nerves.

PROFESSOR MEYER undertook to demonstrate the power of the reproduction of nerves, the possession of which faculty by these organs had before been denied.* He assumed the following tests, as the proofs of the identity of the production with the nervous structure. 1st. Nitric acid, according to the observations of Reil, destroys the neurilema, but has no action on the medulla of a nerve. 2d. The functions of a nerve are destroyed by its division, and can only be resumed by the restoration of the continuity of the nerve *by a similar production.*—If this latter test may be relied on, it is perhaps sufficient for all the purposes of pathology.

Eight lines of the sciatic nerve of a dog were removed on the 29th of July.

Sept. 23. The extremities of the nerve were separated by a distance of only six lines, and they were rounded off by a bulbous extremity. The superior and inferior portions had

* Essai sur la Regeneration des Nerfs, par J. C. H. Meyer.

therefore in this time approximated two lines, to which must be added perhaps a space of retraction of two lines more, so that the approximation of the portions of the nerve was about four lines in eight weeks. The metatarsal bones became exposed, and were withdrawn by the animal.

Aug. 5. A sciatic nerve was divided with a pair of scissors. The limb gradually regained its sensibility; and on the 10th Sept. the animal died. A medium of new growth, resembling the nerve, but rather smaller, was produced from the bulbous extremities. This medium was immersed in nitric acid, which destroyed the cellular tissue, leaving the medulla; and the identity of the production with the nerve itself, confirmed by this double test, was assumed to be demonstrated.

Sept. 26. The left sciatic nerve of a bitch was divided with a scalpel. A difficulty of using the leg succeeded, and the integument covering the toes became excoriated.

Dec. 11. She could walk equally well with all four legs; but the powers of the injured limb were not so perfectly displayed in running. The extremities of the nerve were united by cellular substance, in which was discovered by

the nitric acid a nervous fibril, which established the communication between the two portions, from the superior of which it appeared to originate. These experiments were repeated frequently, and the general fact of the re-union of nerves appears to be indubitably confirmed by the result.

The portions of the nerve were approximated in one instance in the shape of a cone: the tibial nerve was found united by one, and the ulnar nerve by two balls. Frogs were also made the subject of these experiments; and this curious circumstance is mentioned, that all of them died within a few days after the division of a nerve, while one survived some weeks the division of the medulla spinalis. Prof. Arnemann believed the nerves to be re-united only by their membranes. The fact was shewn by Meyer to be otherwise, as the medulla was found after the membrane was destroyed by nitric acid.

We are taught to expect by the result of this examination, that the healing processes will be exerted in the cases of wounds of the nerves, in the same manner as in the mixed structures. But we are furnished with no clear examples of the fact by the histories of surgery: it therefore appeared desirable to obtain an illustration of

the mode in which the continuity is restored, and of the structure which a nerve at such place is liable to assume.

With this view I punctured one of the largest nerves of the axillary plexus of a rabbit with a lancet, by which a section was made of about one sixth part of the circumference of the nerve. A spherule of the medulla exuded from the place of the puncture. Seven weeks afterwards the nerve was examined: an elevation of it was perceptible at the place of the injury, the integrity of the surface was re-established, and the swelling resembled a small vesicle on the nerve, filled with a gelatinous matter, which could not be made to disappear wholly by washing, by friction, or by any of the means of removing an extraneous substance. This vesicle corresponded in size with the exudation of medulla which succeeded the puncture. The contents of the vesicle resembled those of the tubuli of the nerve, which looked glassy and gelatinous, when viewed through the microscope. A slight discolouration was evident about the place of the puncture, which was most probably induced by the inflammation which the nerve had previously sustained.

In an experiment, which was not designed for the present illustration, the structure of a

nerve was accidentally lacerated with the forceps: the injury of adjoining parts was much more extensive in this, than in the above instance. Three weeks afterwards, the nerve, at the place of laceration, was found to be surrounded with dense adherent animal substance, from which it was inseparable. This adventitious growth appeared to be incorporated with the nerve, as if it had been jointly produced from its own, and from the contiguous, substance. Hence it appears that the continuity of a punctured nerve may be restored by the processes of regeneration, but that it undergoes in such situation a change of its natural structure; how far this change is liable to interrupt the functions of the nerve will be hereafter considered.

ARTICLE IV.

Relation of Nerves with their Centres.

A limb is rendered incapable of either sense or motion, by the division of its nerves. From this fact it is concluded that these properties are dependent upon the centre of such nerves, as upon a *source* from which they are obtained.

The conclusion of this dependance appears to require no further support than that which is afforded by the solitary fact;* but the inference does not stop here; it proceeds beyond the evidence of the fact, and assumes, as sensation is no longer excitable at a remote distribution of a nerve, after the division of its trunk, that sensation does not take place where the means are applied which should produce it, but that its seat is in the brain.

As the fact does not comprehend this part of the inference, we cannot be said to possess the evidence which compels assent: and as the testimony cited in behalf of the conclusion is not adequate to prove its truth, it is not required that its refutation should be attempted, but that the force of the evidence should be exposed.

* As the function of the remote parts of nerves ceases after a division of the trunks at various contiguities with their centres, the origin of their powers has been fixed in the brain and spinal marrow. This inference is strongly indicated: but as the nerves assume a different structure at the place of communication with the brain, it would be desirable to ascertain some facts relative to the effects of the division of nerves at the point of junction with their centres, where the neurilema has ceased. In speaking of the acephali (under the title of Relation of Nerves with the Heart) a similar remark will arise out of circumstances with which it is intimately connected. This observation is to be kept in view whenever the dependance of nerves on the brain is referred to: and whether the locality of the origin of faculties is rightly assigned to this viscus, or belongs to the central parts of nerves themselves, the argument will be reconciled by a nominal change.

If the nerves derive from the brain a capacity for sense, and if this capacity exist wherever sensation is displayed, then the alliance of this capacity is with every part of the nervous system, and in its different seats it requires only that the causes should be operative by which the faculty is excited, and sensation consequently produced. But the source of this property is in the brain: if therefore the communication between the remoter nerves and the brain be intercepted, the *presence* of the faculty ceases, the communication of it being prevented. The causes, which during the integrity of the organs produced sensation, are now not recognised, as the presence of the capacity for sensation upon which they act is precluded by the division of the medium of intercourse. There are other circumstances alledged, the weight of which it is also necessary to examine.

We have a clear proof, it is said, of the erroneous reference which we make of the seat of pain in a very familiar example: a blow on the ulnar nerve induces the impression of an injury sustained in its remotest distribution. In this case, which is an exception, we admit the validity of the testimony of the senses, but we refuse to acknowledge it in the extensive range of general occurrence. The senses inform us

correctly in this, as in other instances: we are taught by them to refer the sensation to the place where it is *felt*.

But it will be inquired, why should the sensation take place at a point *remote* from the injury? If it should be replied we cannot answer this question, we confess only that there is an absence of information; an information which must govern the conclusion: surely it is not admissible to assert the conclusion, while we admit that the knowledge which can alone decide it is not attained. We are not without a parellel to this *effect* of an injury manifested at a distance from the place at which it is inflicted, in the perceptible changes which are produced under circumstances sufficiently similar for the purposes of the analogy in the mixed textures, as well as in those belonging to the nervous system. A testicle swells from irritation of the urethra; and I have found an injury of a sciatic nerve which destroyed its animal functions, produce an abscess in its course at a considerable distance below the place of such injury: the sloughing also which sometimes succeeds to an injury of nerves, frequently commences in the extremity of the limb.+ Why, it may be asked, should these occurrences take place? why should such an effect of an injury, influencing the textures, take place at a point

+ and pain in the shoulder when the seat of Disease is in the Liver and [illegible] [illegible] referr'd to the [illegible] when the Hip joint is the pa[illegible]

which is remote from, rather than at one which is immediately contiguous to the seat of the injury? It must be replied our knowledge does not explain this circumstance: we must make the same confession in regard to the remote affection of the functions of a nerve; and the information in which we are deficient will be defined to consist in some secret relations between the parts of this system, which the industry of inquirers has not yet developed.*

In support of the assumption that the brain is the seat of sensation, it will be further remarked, that it is common after the amputation of a leg, for the subject to complain of pain in the toes. But this circumstance likewise agrees with the supposition that the faculty of sensation is derived from a centre, and in connection with the nervous organs pervades all the parts of an animal body. This deception is produced physically; and if we were to separate the causation of the pain from its material connections, we should perhaps find that there was no deception in the case. It is however not necessary to trace this occurrence so minutely; it is required only to show that the fact is no proof that the impression of pain in the toes is

* When this relation is referred to by pathologists it is termed sympathy.

produced by, and has its seat in the brain, which will be evinced if the circumstance admits a different explanation.

If the capacity for sensation is imparted from the centre to the extremities of a nerve, this capacity must be present in the trunk before it is possessed by the branches; if it be excited in the trunk, the effect, the sensation, will be produced in this place. But it is not a natural office of the trunks to furnish the same sensations as the branches: we must therefore conclude that the properties which are derived from a nervous centre to be distributed to the extremities, are modified in their course. If the trunk were affected by disease, the consequent modification of its function might lead to the same phenomena (operating on the same derivation from the centre) as those which in the condition of health are produced by the branches. Thus we see in the arterial system, the pellucid extremities sometimes carrying red blood; we see the secretions from the same set of vessels becoming variously depraved; we see the surface which at one time exudes a serous fluid, at another discharges pus, and then throws out coagulable lymph.

The pain after amputation, therefore, which is supposed to proceed from the toes, may be

explained in a way which is consonant with all the facts; indeed the explanation scarcely exceeds the facts, which will admit the locality of sensation to exist where it is referred. In consequence of the section of the nerve, the cut extremity inflames: the natural office of this part of the nerve is modified by the disease which has commenced in it; under the influence of this disease the function of the extreme branches is assumed; the faculty of sensation being present in the cut extremity is thus excited, and an effect is produced, which corresponds with the local deviation from health.

It is with regret that I enlarge upon this discussion, because the prosecution of it is rather a matter of speculation than of practice: at the same time there are a few points which it is essential, if possible, to ascertain, and which it is therefore proper to notice in this place, provided our reasonings proceed no farther than they are capable of deriving some sanction from facts.

Although it is not clear that sensation takes place in the brain, we find it necessary to acknowledge that this viscus is a common centre of impressions. We are furnished with the proof of this inference by the series of actions, and by their complications, which result from

an agency upon one sense. Thus the soldier *hears* the command of his officer, and he presents his musket, or advances on the charge: the communication is not *direct* between the auditory faculties and the voluntary muscles, but it is through the medium of the brain: or he *sees* the arrangement of battalions, and *listens* for the signal of attack. In these instances there is a reciprocal affection between the senses; and a series of interchanges of the animal powers take place, the organs of which have no mutual connexion, except through the medium of the brain.

We must not however mistake impressions for sensations: there is no more reason for supposing that the seat of these sensations is in the brain, than that the motions of remote voluntary muscles, which result from them, and which are also dependent upon the same viscus, are performed by the central organs.

The function of the brain is exerted from itself outwards to remote parts; that of the nerves from themselves, or the places of their affection, towards the brain; we have an instance of the former in the acts of volition, which are secondary; and of the latter, in the primary impressions which induced them. It is true that the evidence of the senses appears to be sometimes corrected by the judgments of

the understanding; but in this we are deceived, for the act which is here referred to is the correction of the inference, which is furnished by one sense, by the increase of evidence which is acquired by another, each sense possessing its peculiar objects of cognizance.

The dependance is reciprocal between all the parts of the nervous system; it is either direct or mediate, and consists either of privation, or increase of properties.

The properties which the nerves derive from their centre, are necessary to sense and motion: these properties are not however the faculties themselves, which is more conspicuously evinced in the organs belonging to the senses; but they are equally constituted by others, *which result from the function of the nerves.* It is not natural that the superior portion of an intercepted nerve should exercise the capacities which are dependent upon its ramifications: if, for example, the trunks of the digital nerves were divided, the faculty of touch could not be exerted by the cutaneous filaments which proceed from the trunks above the place of the division; nor would the exposed surface of a divided optic nerve be sensible of the presence of objects. These faculties are therefore not an emanation from the brain, which is alike possessed by all

the parts of the nerves, but are produced by the offices of the nervous extremities, with the concurrence of an influence from the brain, both being equally necessary to the end.

If it were the business of a nerve to propagate only one homogeneous property derived from the brain, we should have no diversity of phenomena in the different parts of the same nerve, and the division of a nerve for the cure of its diseases (occupying an inferior seat) would be perfectly futile and useless.

As the nerves are to be considered a system composed of many parts, so also is the brain one whole, the distinct functions of which admit an analytical reference to the parts by which it is constituted. These parts have been termed organs, between which the same reciprocation of function subsists, as is to be more diffusedly met with elsewhere. The cessation of one set of phenomena, and the continuance of the rest, sanction this belief of the distinctness of a function which belongs respectively to the portions of the viscus. Disease of the central organs will sometimes be evinced by the destruction of one set of capacities (as illustrated in the various instances of pressure from fluid), and disease of one nerve (as one of sense) will render extinct the phenomena which the brain

would otherwise elaborate: the prevention of the operations of that part of the brain which is connected with such nerve will influence the results of the exercise of all the other organs. The perfection of the faculties of sense and motion requires a perfect condition both of the nerves and of their central terminations. Disease in either interrupts or destroys these faculties.

The natural condition of the brain which is necessary for these faculties, and to which these faculties are superadded, is, 1st, The presence of a perfect principle of life; 2d, The aggregation of its proper matter; 3d, The arrangement of this matter in consonance with laws which from their minuteness and obscurity are not understood; 4th, The presence of arterial blood.

The blood has with the brain a double relation; one might be termed hydraulic, and the other respects the properties of the fluid. The proofs of the former are, 1st, That a considerable reduction of the volume of arterial blood, circulated in the brain, is sometimes attended with death; and, 2d, That its function is liable to be interrupted or destroyed by a preternatural fulness of its vessels. That the dependance of the function of the brain is upon the properties as well as upon the quantity of its blood, is shewn by this fact, viz. that the processes by which the

blood is perfectly formed, and by which its properties are conferred, cannot be either remitted or counteracted compatibly with the continuance of the function of the brain: thus the prevention of the office of the lungs is followed by the death of the brain; and thus its function is made to cease by the injection of air into the carotids. *

The conditions which have been remarked as essential for the contribution which the brain furnishes towards the faculties of sense and motion, are equally indispensable, in regard to the constitution of the nerves, for the fulfilment of the same end; they may also be extended to the medulla spinalis; and they obtain in every part of the nervous system. The lower extremities are paralysed by a ligature on the abdominal aorta; and a voluntary muscle may be incapacitated by the accumulation of venous blood. To enumerate the other conditions, would only be to repeat the particulars just mentioned, which are those of the analogy. These conditions correspond with the sources of primitive affection; as their functions are relative in health, the reactions are complicated which take place under disease.

If we are called upon to consider the *mode* in which the sensations excited in the nerves are conveyed to the brain, and that in which the

* The cerebral functions will cease, if venous blood only is carried to the brain

impulses of volition are propagated to the organs which obey them, we have indeed a field upon which ingenuity may be exhausted, and the efforts which might be better employed engaged in a fruitless attempt. I shall notice therefore, and that briefly, only two opinions, viz. the one which assigns the presence of a principle, the rapidity of whose motions may be coarsely illustrated by a comparison with the electrical matter; and the other, which assigns the presence of no properties in the nerves, save those which are common to inanimate matter, and are capable only of vibratory actions. It appears to me that the first is confirmed by the facts by which the second is refuted: of these facts I shall mention only the following.

1st. The properties of a nerve are different at different places in its course: thus, the 8th pair animate the organs of voice, and, according to the experiments of Mr. Brodie, are essential to the secretions of the stomach. If these actions were mechanical vibrations, every collision which a nerve sustains should be a cause of effects which depend only upon vibrations; and death itself should be only a state of rest of the organs, whose actions should be renewed by the vibration of their proper nerves.

2d. If the affection of the superior portion of

a nerve were comprised in a state of vibration, which is extended from the inferior portions, the former, after a division, when exposed and struck, would certainly be in the same state as under the condition of the integrity of the chord; we cannot but suppose that it would vibrate, when the facility of the vibration is assumed as the basis of the doctrine: thus the exposed cut surface of the trunk of the optic nerve should distinguish the presence of objects the more readily, as the impulse of light upon it would be stronger at the place of division than when the force of the impulse was diffused and expended upon a greater range of structure, as in the natural state. This should be the case if the coats and humours of the eye have no other effect but to regulate and arrange the rays of light, thereby preventing the confusion of objects: but as the faculty of vision ceases when the anterior part of the eye is cut away, although the retina is exposed to the rays of light, I believe that the *properties* of light are changed by passing through the coats and humours; and that the processes by which this change is accomplished, are preparatory to a further act of causation, which takes place between the light and the properties of the retina, and which *constitutes* the immediate perception of objects. Of these preparatory processes we have many examples; for though nerves have

independent functions, yet the whole system is a tissue of subordination, and the end of this great concurrence is the perfection of an animal body.

3d. The pressure of a dislocated humerus upon the axillary plexus, has been known to paralyse permanently the powers of the arm. Here there was more than Prof. Arnemann required, in order to perform the functions which are dependent upon the brain and nerves: here there was not only the continuity of an animal chord, but that chord was a nervous structure.

4th. The application of a ligature to a nerve destroys its faculties of sense and motion: in this case, likewise, there is the continuity of an animal chord; for there is no interruption of the entireness of the neurilema, which is only compressed. It is proved by this circumstance, that the continuity of the *medulla* is necessary to the perfection of the function, and that, that of the membrane is inadequate to this purpose. Will it be said that the presence of the ligature, as a foreign body, by its weight, &c. impedes the vibratory motions? We cannot admit this, when we recollect that tumours of considerable size sometimes form in the trunks of nerves, without a destruction of the sentient faculties of the extremities; and, further, that the paralysis continues after the ligature is removed.

Similar facts may be greatly multiplied, but it appears superfluous; and, upon a comparison of all the circumstances, the most reasonable conclusion seems to be, that the centres of the nerves furnish common properties to respective nerves; that every portion of a nerve which displays an additional or a different property, has also a distinct function; that this function co-operates with the influence from the centre, which influence it is necessary that every part of the nerve should possess; that by this concurrence the diversities of phenomena are produced; and that when the local function of the nerve is impaired or ceases, or when that of the centre is disordered or suspended, then the conjoint function is either modified or at an end.

Sensations take place in the nerves: the brain is impressed by these sensations, and a corresponding exertion of its own faculties succeeds. These are only different stages of a continued causation; and the processes are still further complicated by re-actions. The order of these occurrences has been differently indicated, and the appellations differently appropriated in the works of some systematic writers, but their adoption has been attended with no great success.*

* Some additional facts relating to the connection between the nerves and their centres, will be noticed in the subsequent inquiries, which respect more particularly the pathology of the nerves.

ARTICLE V.

Relations between Nerves.

THAT the respective nerves are liable to be influenced in consequence of the relations which subsist between themselves, as well as by the participation of their centres, is indicated by many facts. The facts to which I allude, are those which are comprised in the detail of the sympathies; of the instances of which we are all sufficiently informed.

By the word sympathy is meant a sensible affection of a structure, generally if not always occupying the nerves, at a distance from the place where the cause is applied, and where the process of the affection originates.

Two alternatives suggest themselves, of the mode in which a sympathy takes place, viz. either by the metastasis of the properties affected at the place of origin, to that which is remote; or by the communication of an impression from a seat of properties which are *indisposed*, to one, which, from difference of function, or an occasional cause, is *disposed* for the affection which is

identified in the effect. Without making an attempt at the decision of this question (the connections of which are highly important in practice), I shall merely offer a few observations and facts, which have with it an intimate alliance.

A portion of one of the largest nerves of the axillary plexus of a rabbit was destroyed, by which the animal was rendered lame; seven weeks afterwards no perceptible lameness remained, and the recovery of the powers of the limb up to this time was gradual: the wound had healed in less than three weeks, and as the restoration of the natural powers did not succeed for a considerable time the healing of the wound, the lameness was not to be referred to the injury inflicted on the muscular structure. I expected, from the facility and strength with which the motions of the leg were performed at the above period, that the re-union of the nerve had taken place. On examination, however, I found the extremities of the nerve to be totally unconnected with each other for the extent of half an inch.

In many subsequent experiments, as well upon axillary as upon sciatic nerves, I found that an improvement, or perfect recovery of the motions of the limb, took place before sufficient time had elapsed for the re-union of the nerve.

Dr. Haighton has given us a masterly investigation respecting the proper nerves of the voice; in which he demonstrates that this faculty is principally dependent upon the influence of the recurrent nerves, and subject to be but very slightly affected by a division of the superior laryngeal nerves. The loss of voice was alike produced by a division of the recurrents, or of the par vagum; and it succeeded to a division of the eighth pair, below the place where the superior laryngeal nerves are given off.

One of these trunks was divided in a dog, which rendered him hoarse: in a *few days*, "his voice *approached the natural pitch*, but considerably fainter than before." The other trunk of the par vagum was divided six weeks afterwards; a total loss of voice succeeded, which lasted about a fortnight: the faculty was shortly after resumed, though "the pitch was sharpened almost an octave:" the strength of the voice continued afterwards to increase. Thus the voice is destroyed by a division of its proper nerves, and is restored when the re-union of one of them has taken place. It is thus shewn that an increase of the power of a sound nerve may succeed to the privation of the offices of one which is auxiliary: the seat of this increase of power is not ascertained.

[illegible] one Faculty is abolished — others
[illegible] continued & increased Exercise
become much more acute — and
[illegible] the extent for every [illegible]
impulse — the Deaf & Blind are famous
examples of this kind — will a ratiocination
[illegible] of this sort apply to the above?

But the experiments are continued further; and they go on to shew that this metastasis is not complete. This is evinced by dividing one of the nerves of the voice, by waiting until the natural pitch is restored, and then by dividing the same nerve again, which reduces the voice to the state of it after the first division of the nerve; thus proving that the restoration of the natural voice is dependent, not upon the assumption of the faculty of both nerves by one of them, but upon the re-union of that which had been divided.

These results do not exactly correspond; and the criterion, viz. the restoration of the former voice of the animal, is not perhaps to be confidently relied on, as the comparison cannot be made with precision after a considerable interval of time has elapsed. But the results of Dr. Haighton's third experiment prove that though the sound nerve may be inadequate of itself to a *perfect* vocal function, yet an augmentation of its powers is obtained when the assistance of its concurring nerve has been prevented by division. We are also led to suspect a more general metastasis to take place in animal bodies, under some of the conditions of disease.

From exposure to the weather or some other sudden cause, an accession of fever takes place;

and, in less than twenty-four hours from the period of perfect health, the muscular powers of the subject will be so reduced, that it is with difficulty perhaps he is able to support himself. At this time the action of the heart and arteries will be extremely violent. The febrile diathesis continues, and for several days is gradually augmented: visceral inflammation may perhaps take place in the course of the disorder; and as the vital powers are raised in one place, or in one set of organs, a proportionate debility will obtain in the faculties of the voluntary muscles. This increase on the one hand, and reduction of power on the other, will continue for many days, weeks, or months; and finally, the same celerity of the circulation shall prevail (as in phthisis) up to the period of death, previous to which event the powers of locomotion shall have become almost extinct.

It will be said that the connection which is thus indicated, should be supported by the converse of this occurrence. If such a metastasis is capable of taking place, the increase of voluntary action should, by the expense of voluntary power, produce a depression of the pulse, and debility in the organic system. Furthermore it will be urged, that a state of hemiplegia, where the paralysis occupies one half of the body, should be invariably accompanied with an in-

creased vigour and celerity of the circulation, while there is in fact no perceptible difference.

These objections must in this case be inapplicable: the first, because exercise increases the momentum of the blood, which re-acts upon the irritability of the heart; and both, because we are supposing the conversion of one set of powers, or those of one order of structure, to the purposes of another, not as a general occurrence, but as one which is liable to take place under certain conditions of disease.

We have likewise an argument deduced from temperaments to support the conjecture. We observe that delicacy of constitution, and but a small share of muscular strength, are generally accompanied with a pulse habitually rapid; an irritability of the heart, subjecting it to violent palpitations, under the actions of slight stimuli: while those of a coarser growth, of greater development of the textures, and possessed of great muscular strength, have commonly a full and slow pulse, which is scarcely to be influenced by any cause which interests the affections.

An observation in some respects analogous may be extended to the energies of the mind: there has been remarked, among other indications of a tendency to consumption, a premature

development of the intellectual faculties, accompanied with a weak constitution and slender growth. The connection is still further exemplified by the fact, that a dog, hunted after a full meal, having the voluntary powers put in vigorous action, and the instinctive interest of the pursuit strongly excited, will not digest the food which he has taken; but it will remain in the stomach, a crude aliment, until rest permits a sufficient concentration of the vital powers, for the purpose of the conversion of the food to an aptitude for assimilation.

In short, a vast deal of the evidence of association (which, indeed, is very short of proof) may be adduced to support the conversion of one set of faculties, upon fixed conditions, to the objects of another; but if the connection of cause and effect should in any instance be established, it would prove but an useless attainment as long as we remain ignorant of the precise relations of faculties with each other, and with the textures: of the changes which these are liable to undergo, and of the means of a reasonable anticipation of the effects of such changes, in a whole circle of connections, in consonance with some known laws. This notion of the metastasis we are considering, might have suggested the medecina gymnastica, once so strenuously insisted upon, and now so totally neglected for

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a nonsensical farrago of drugs and sweating machines. I cannot help suspecting that this inquiry might be reduced to one of experimental certainty; and that its results, whatever they might be, must be beneficial to medicine.

But the question here to be considered, is not with respect to a metastasis of the powers of the animal, to those of the organic, life; but whether, in the animal system, the offices of an intercepted nerve may be assumed by one, whose communication with the centre is unimpeded. That such an increase of power does take place to a certain extent, is demonstrated by the fact, that an animal, rendered lame by the destruction of a portion of one of the nerves of the leg, recovered from that lameness, while the extremities of the nerve were still separated by a considerable distance.

In a case of disease of a digital nerve, related by Mr. Abernethy, the sensibility of the finger was restored in three months, though a half an inch of the nerve had been removed. In one of the experiments of Meyer on the sciatic nerve of a dog, an approximation of four lines of the portions of the divided nerve took place in two months:* this growth is in the proportion

* The same proportion of growth obtained also in some other of his experiments.

of half an inch in three months. It is probable that the re-union of nerves may be facilitated or retarded by circumstances which we cannot reason upon or anticipate, but the traces of which we perceive in phenomena.

When a considerable interval has elapsed between the destruction of the continuity of the nerve and the recovery of its powers, nothing short of an actual examination of the state of the nerve can sanction us in the inference, that the recovery of the natural powers of the nerve is in any way independent of a restoration of its continuity.

In order to prove whether the restoration of the power of an intercepted nerve (for this restoration takes place to a certain extent) is dependent upon an increase of function, assumed by the sound nerves, or whether a nervous power is communicated from sound nerves to one which has been intercepted by the communications of branches, I thought it desirable to obtain, by further experiment, an increase of the facts which have a reference to this question; and the more particularly, as I am acquainted with no investigation which applies directly to the point.

With this view, I exposed the lower portion

of a sciatic nerve (five days after it had been divided, and some recovery of its powers was indicated) for the extent of an inch: it was cut, pinched, and lacerated with the forceps: the animal *did not evince the slightest sensation*, but he started if the superior portion was in the least degree irritated.

June 5. I applied two ligatures on the sciatic nerve of a rabbit, and divided it between them. I preferred the ligatures to a simple division, in order to provide against a speedy re-union, which would have frustrated the design of the experiment.

August 5. A considerable improvement of the voluntary powers of the limb had taken place; so great indeed, that I was fearful my care to prevent a premature re-union had been without success; but in this suspicion I was mistaken. I exposed the parts of the same nerve, and continued the dissection near two inches below the place of its division. I then applied a ligature to the most inferior point of the lower portion, but *the animal did not evince the slightest evidence of sensation:* the nerve was divided by the scissars with the same result. If the superior portion was pinched, or irritated, a lively sensation was manifested.

This experiment appears to be as conclusive as any which will readily suggest itself, on a very important point of the pathology of the nerves: without such a decision, every thing which we can devise for the improvement of the practice which has been instituted for the cure of some diseases of the nerves, must be futile and nugatory; for if it should be proved, that the sensibility of a nerve is acquired below the place at which it is intercepted, by communication with sound nerves, we have but little to hope from the success of a project, which is calculated to prevent permanently the transmission of central influence. But if the improvement of the faculties of the nerves had taken place by derivation of nervous power from sound nerves (in some measure analogous to the effects of the anastamoses of the arteries), the sensibility would surely have been excited by the application of a ligature, which never fails to produce exquisite pain in a nerve which is not *deprived* of its sensibility.

We must conclude therefore (if the sufficiency of these data be admitted) that the improvement of the nervous function of a limb, previous to the reunion of one of its divided nerves, results from the assumption of an increased power or energy, or an increase of the properties, belonging to the sound nerves. It is sufficient for the general

purposes of this branch of surgical pathology, that we can positively deduce this inference in regard to the *sensibility* possessed by the nerves: it is probable that the same conclusion may be extended to all the other faculties belonging to these organs, which are derived from their respective centres.

ARTICLE VI.

Relation of Nerves with Muscles.

SO intimate is the dependance between these parts, that the muscular fibres have been considered as the extreme filaments of the nerves. The power of retraction possessed by the nerves has been before remarked; and this power has been adduced, as furnishing an explanation of the mode in which a muscle acts. The action of a muscle has been described as ensuing from the contraction of the nerves, by which a corresponding contraction takes place in the muscular fibres, whether we consider them to be parts of nerves, or as fibres of independent formation, attached merely to the nervous extremities.

I have only to remark on this question, without considering how far the fact is conclusive, that the belly of a biceps muscle, which, in the extended state of the fore arm measures 6½ inches, measures less than 4½ inches, after it has assumed that contraction by which the fore arm is bent. As the lines of the muscle are conspicuous in either condition, it admits of being measured with tolerable accuracy. It must here be observed, that the stimulus of electricity will produce no increase of the spontaneous retraction of a divided nerve, and that this retraction has been found not to exceed ^{7} of an inch: the disparity of the decurtation, in the two instances is strikingly evident.

The muscles cease to be animal organs after the division of the nerves by which they are supplied: they are nourished, and they preserve an organic existence; but they are rendered incapable of the distinguishing characteristics of sense and motion. It has been supposed, both from the observations of disease furnished by practice, as well as from some apparent distinctions of physiology, that some nerves are productive of sense, and others of motion; or, in other words, that the different phenomena of the nervous system are accomplished by different nerves, and that the individual nerves possess only one faculty. This opinion is made upon

very easy terms: it was conceived that it might be so, because there are no apparent proofs to the contrary. That the nerves belonging to some of the senses are not subservient to motion, appears very probable; as in the instances of the optic, the olfactory, and the auditory nerves: whether an exclusive function is possessed by the others, is perhaps yet to be determined. But that some individual nerves may exercise both the functions of sense and motion, is proved by this fact, which will hereafter be fully exemplified, (viz.) that the division of a nerve of the arm, as the *radial* for example, will render a great portion of the fore arm *insensible*, and will incapacitate the muscles of the limb for the performance of *voluntary motions*.

The muscles are the instruments of the nerves, as these latter are of the properties upon which their phenomena ultimately depend. The spontaneous affections of the material organs are never primitive; but they are liable to become reagents.

That the muscles constitute an order of structure distinct from the nerves, appears to be indicated by this fact, which was ascertained by Fontana, (viz.) that laurel water is innocuous, though liberally applied to an exposed nerve; while it is as certainly fatal, when applied to a

wound of a muscle. The conclusiveness of this evidence would be increased, by ascertaining the result of the application of the same poison to a wounded muscle, after a division of the nerves of the limb.

The muscles display many curious phenomena, which it is unnecessary to mention: they are principally comprised in varieties of sense and motion, and are therefore dependent upon the integrity of their nerves, as well as of their centres; the muscles co-operate in these effects with the force of material organs of apt composition and arrangement.

The subject of muscular motion is one which has given rise to many theories, and consequently to much discussion. But it does not appear necessary to notice these theories, any further, than to remark, that the evidence cited in their behalf is in general very short of proof, and that they are not therefore to be admitted as knowledge. The opinion of Girtanner, by which it is attempted to explain muscular contraction by a supposition of chemical combinations, is one of some ingenuity, but is very inadequate in its extent, and not confirmed by the data so far as it is indulged. The attempt, however, may be useful, if it lead to a more rigid inquiry.

According to Bichât, a voluntary muscle displays four kinds of contractility: animal contractility, as when it acts at the instigation of the will; sensible, organic contractility, as by the action of a chemical, or other extraneous stimulus; insensible, organic contractility, as by the impulse of its fluids, constituting tone; contractility of tissue, as by a transverse section of its fibres, arising, as it is said, from failure of extension.

ARTICLE VII.

Relation of Nerves with the Lungs.

THE connection between the function of the nerves, and that of the lungs, has been considered only in respect to the communication of the former with the brain. The inquiry on this subject has been very extensive; and the results are still involved in some theoretical discussion, and still opposed by the differences of experiment. It is however ascertained, that the mechanical processes of respiration cease, upon a division of the nerves which supply the lungs; while the chemical may be in part, if

not wholly, maintained. The change of the colour of venous, to that of arterial, blood, is not prevented by a division of the par vagum and sympathetics; or by decapitation, provided the mechanical processes of respiration, by which the presence of air in the lungs is admitted and regulated, are artificially supported.

Among the chemical phenomena of respiration, has been mentioned the generation of animal heat; but as a considerable discrepancy of opinion prevails on this subject, and as an attempt at the increase of the facts belonging to it has never made a part of my inquiries, I omit to offer here any further remarks upon the question, reserving a few observations on the circumstances of the temperature of animals, as connected with the nerves, to a future part of the subject.

ARTICLE VIII.

Relation of Nerves with the Heart.

THE question which respects this relation, has hitherto been, whether the action of the heart is independent of the brain? This question has been variously answered.

It has been assumed from the histories of the acephali, as the circulation has been ascertained where no brain has been present, that the function of the brain is not necessary to the action of the heart. But this is deducing a general conclusion from an exception to general occurrence; and even those who have adopted the opinion upon this presumption, have admitted that the influence of the brain has been necessary to respiration, which has also been performed in fœtuses without brains. Indeed, if these cases are of sufficient validity to sanction a general inference, the brain is a viscus, which is almost superfluous in the economy of *animals,* as will appear from the leading particulars which are expressed in the histories.

Kerkringius mentions the case of a boy, *five months old,* in whom only a slimy fluid was found occupying the place of the brain. Dr. Heysham reports an instance of a female child, in whom there was no trace of a brain: this child lived six days; and the place of the brain was found to be occupied by innumerable cysts, containing a brownish fluid. The organs of the senses were in this case exercised, and perfectly developed. Dr. Simson records an instance of the total ossification of the brain of an ox; and adds, that the axe of the butcher penetrated this mass, without occasioning any deprivation of sense. Dr. Monro has mentioned the case of a monster of singular conformation: the lower parts of the body were of the natural size; but it was deficient both of the head and neck, and many organs which are otherwise essential to life. The spinal marrow was of a conical shape, terminated in a canda equina, and sent off eighteen pairs of nerves of the usual size.

Mr. Barlow has given an account of a case, in which the brain, *spinal marrow,* and optic nerves, were deficient. A membrane occupied the place of the cervical, and the greatest number of the dorsal, vertebræ; and, on being slit open, it was found to contain some fibrils, which might be regarded as nerves, but no substance correspondent with the medulla spinalis was

discovered. Soemmering observes, "Non raro spina medulla est sine cerebro, nunquam vero cerebrum sine spina medulla est visum, deficiente enim spina medulla cerebrum simul abfuit." Mr. Barlow concludes that a fœtus having no sensorium (brain I presume) can possess no faculty of sensation. The use of the nervous centres in the embryo is, from this and other cases, rendered dubious.* To hazard a conjecture upon the anomaly at this time, would be to anticipate thoughts which should be exhibited in another series; I cannot, however, dismiss the topic altogether without a suggestion.

If there be a defect of the organic spirit which produces the brain, but no defect of the powers conferred ab origine of sense and voluntary motion, the latter will exist in the fœtus, and the former will not. As every faculty exercised by an animal is allied with some material organ (for obvious purposes); and as the faculties have not an indiscriminate alliance, such as voluntary power with the membranes, digestive power with the voluntary muscles, &c.

* Cases of acephali are abundantly scattered in the records of medicine: those who wish for the details, may find them in the writings of Diembroech, Ridley, Bartholine, Bonet Sepulchr. in the memoirs of the Academy at Paris an. 1703, in the Transact. of the Royal Soc. of Edin. in the Lond. Med. Journ. for Sept. 1800, in the works of Monro, Clarke, Sandifort, Soemmering, &c.

but are connected acccording to the relations which subsist between faculties and organs; so the powers of sense and motion existing in the fœtus, will be allied with the organs for which they entertain the strongest affinity: if the brain be developed, this, as is indicated by the facts, will be their seat; if the brain be not developed, from defect of that which I call its organic spirit, the faculties will be allied to organs bearing a relation, though of a less perfect, or of a modified, kind; and the facts, that the acephali, possessing nerves which are not connected with their common centres, and exercising the faculties in question, prove that when the brain is absent, the faculties which otherwise appear peculiar to it may be adopted by the nerves, and these organs rendered at once the instruments and the sources of their own powers.* But that this assumption of office is not one of a perfect kind, is proved respec tively by the sequels of the cases.

* This is the only explanation which occurs to me, if it be allowed, as it commonly is, that the brain in the well formed subject is the *source* of the faculties of sense and motion: but that such is the case, is not demonstrated; for the heads of nerves may be the *natural sources* of these faculties: the function of the brain may be rather of the intellectual kind (which is to a great extent conformable with the grades of animal life, considering man as the most perfect example), and its injuries may be attended with an impairment or suspension of the function of the nerves, in consequence of a reciprocal capacity for affection which subsists between them. For the present, however, as there is a deficiency

Whether this relation of function with the organs might be accompanied with any change of sensible arrangement, occupying (or perhaps contigious with) the heads of individual nerves, is a point which may escape the scrutinies of the anatomist: but the question is not essentially connected with the explanation; and whether the circumstances are to be explained in this, or in any other way, it is very obvious, from the nature of the facts, that the inference of the independence of the heart of the brain does not derive a testimony from this source which entitles it to implicit belief; nor must we give up ourscepticism on this particular, until it is shewn incontrovertibly that the brain is a superfluous viscus in the *animal* economy, and one which might be dispensed with, without any loss.

It is affirmed by Bichât, that all the nervous connections of the heart may be torn away, and the action of it will still continue. More than a hundred years ago, it was shewn that the action of the heart might be maintained after

of facts on this subject, I shall say nothing further on behalf of these suggestions; and shall continue to speak of the brain, according to common opinion, as the *source* of those powers in the nerves, which are essential to the phenomena of animal life, meaning in strictness, by the expression, just so much as is demonstrated, (viz.) that an intercourse with the superior parts of nerves, that is, with those nearest the centres, is necessary for the purposes of sensation and motion, in their extremities.

decapitation by artificial respiration: the same result was frequently confirmed by succeeding experimentalists; among the most recent of whom is Mr. Brodie, whose experiments I shall quote as sufficient to refute the inference they were designed to prove. In the report of these experiments, we are informed that the actions of the heart were maintained after decapitation, by artificial respiration, as long as twenty-five minutes; at which time "these actions became fainter; and the experiment was put an end to." It is hence concluded, as the action of the heart ceases in twenty-five minutes after decapitation,* that this action is independent of the brain! It is obvious that the same means should have produced the same effect for an indefinite length of time, if these means *comprised all that was necessary to the end.*

In refutation of the opinion of Haller, which supposed the heart to possess an inherent irritability, which enabled it to perform its motions in consequence of the stimulus which ensued from the presence of the blood, M. Le Gallois has recently given the report of a very ingenious investigation.

* It is presumed that an action of the heart, capable of maintaining the circulation, did not continue longer than this time: I have found it cease under the practice of artificial respiration in a shorter time.

It is assumed as a result of this inquiry, that the action of the heart is dependent upon the integrity of the medulla spinalis: this conclusion is deduced from the suddenness with which the destruction of the medulla spinalis kills the heart, although pulmonary inflation should be performed. The other inferences drawn by M. Le Gallois are very numerous; they are made in connection with facts; but they are in truth no better than speculations, which are liable to be opposed by others equally reasonable. I would not be understood to extend this exception to all the results of the inquiry; nor is it necessary to enter here into a discussion which is almost without limits. If the action of the heart be dependent upon the medulla spinalis, the nerves must be the medium of this relation: how then can it be true, that the action of the heart will continue, as Bichât has affirmed, after all its nervous communications are destroyed?

The action of the heart, under these circumstances, is in consonance with the theory of an inherent irritability. According to Haller, the heart rests when its veins are tied: its action is renewed by the impulse of air, by the injection of fluids, or by the removal of the ligatures from the veins. It was hence concluded, that the distention of the cavities of the heart was all that

was necessary for its contraction, and that the properties of the blood were not efficient. This last part of the conclusion accords with the circumstance, that both sides of the heart will contract upon black blood.

The facts which have been collected on this subject are numerous, and contradictory: let us examine briefly the force of evidence on the different points. The subject of consideration is principally comprehended in the following questions. 1st. Does the heart possess a permanent faculty of contraction, inherent in itself, which is excited by the distention of its parietes by the presence of blood? 2d. If the source of the faculty of contraction be not in the heart's own structure, to what other organ does it belong?

1. If the heart be furnished with a permanent faculty of contraction, the effect of this faculty should also be permanent. If the contraction be produced only in consequence of a certain degree of distention, it should not proceed further than a reduction of the volume which produces a less degree of distention; and the contraction should not be continued to the expulsion of a quantity of blood, which is inadequate to excite the faculty by which it takes place. The contraction of the parietes of a ventricle, having reduced the blood it contains to a

supportable quantity, should then be uniform, if the faculty be permanent; for an increase of the quantity of blood in the ventricle must exceed the supportable quantity, and the faculty of contraction being thus excited, would resist further distention. The faculty of contraction, exhibited in the motions of the heart, appears then to be not a permanent power, but one which is possessed, and exerted, irregularly; one which alternates with a power of dilatation.*

2. As the action of the heart capable of maintaining the circulation, ceases in about twenty minutes after decapitation, notwithstanding the blood is oxygenated by artificial respiration, and notwithstanding the integrity of the medulla spinalis is preserved; so the oxygenation of the blood, and the integrity of the

* I have before expressed a similar view of the action of the heart. (Essay on the Absorbents, &c.) Mr. Brodie has since shewn that the heart continues to act after the blood of an animal has been abstracted; a fact which is every day exemplified in the slaughter-house, and which refutes a theory designed to explain the diastole and the systole of the heart, (viz.) that when the ventricles contract, the cardiac nerves are compressed between the auricles and the great vessels; at the same time, that the mouths of the arteriæ coronariæ are closed by the valves of the aorta: that thus the ventricles become paralysed (both from pressure on the nerves and defect of arterial blood,) admitting without resistance the blood from the auricles. That a state of paralysis is not the condition which permits the influx of blood into the ventricles, is shewn by the fact, that the ventricles contract both after the division of the cardiac nerves, and after the circulation has ceased.

medulla spinalis, are not sufficient, under these circumstances, to maintain the action of the heart, by which the circulation will be performed.*

3. As the circulation will continue twenty minutes after the removal of the head, and then ceases, we must conclude that the dependence of the action of the heart upon the function of the brain is not direct, but mediate; that it is not the immediate source of the power which moves the heart, but that it is connected with this source.

4. The modes of this connection are comprised in two alternatives. 1st. Whether the dependence of the function of the medulla spinalis is upon that of the brain? this refers

* This period of the continuance of the circulation, is mentioned as conformable with the result of the following experiment.

Oct. 28. I tied the carotids of a rabbit, opened the trachea, secured within it a tube attached to a small pair of bellows, and decapitated the animal above the ligatures: artificial respiration was practised; and arterial blood was thrown from a puncture in one of the carotids, with considerable force, fifteen minutes after the commencement of the process. At the expiration of twenty minutes, the artery was divided between the ligature and the heart: a little oxygenated blood flowed out, but not with a jet; and the effusion ceased before a dram of the fluid had escaped. The chest was opened, and the motions of the heart still continued. The period of the continuance of the circulation after the removal of the head, under the influence of artificial respiration, may perhaps be variable; in this instance, it ceased in about 20 minutes.

to privation. 2d. Whether the injury done to the medulla spinalis by decapitation is the cause of the suspension of a function which appears to belong to the inferior parts of the chord, inasmuch as the function may be continued for the space of twenty minutes? this refers to affection.*

The decision of these questions is of more importance than that of any other which has ever been attempted for the understanding of the relations of the nervous system. The analogies which should assist us are here equally favourable, and equally conspicuous on either side. The observations with respect to time and order of occurrence furnish but little help: we cannot say that the cessation of phenomena immediately succeeds to the privation of the offices of an acknowledged source, as appears to be exemplified in the division of a nerve; we cannot affirm this argument; as we are assured that organs whose functions depend upon a source, have a capacity for a certain quantity of the influence which is thus obtained, which may last for a variable period: this is exemplified in the heart itself (if the faculty of its

* This word "affection" is not very distinctive; but it is common in medical conversation, and on this account I employ it: I would be understood, by it, the effect of an influence, imparted, or conferred.

action is derived extra se), whose motions I have found to continue a considerable time after the destruction of the medulla spinalis. The alternatives proposed above are strictly conformable with the laws of causation; and the recollection of them will render us diffident of concluding with too much certainty on many other occasions.

5. As the destruction of the medulla spinalis immediately prevents that action of the heart which maintains the circulation, it is thus ascertained that such action of the heart is more intimately connected with the medulla spinalis than it is with the brain: if this connection is that of the dependence of the heart, for the power which animates it, upon the medulla spinalis, then this power is communicated, it acts, is expended, and requires again to be renewed from the same source. But it remains to be shown, in this case, what is the mode of this connection, whether by privation, or by affection?

6. As the destruction of an inferior portion of the medulla spinalis, or even an injury of a sciatic nerve (as will hereafter be shewn), is capable of producing the death of the heart, so the action of the heart might be said to depend upon the sciatic nerve, if every cessation of

phenomena proved a dependence for faculties conferred, as upon a *source;* but we should denominate this an instance of death by affection of vital organs: the line of distinction, however, yet remains to be drawn.

7. As the heart may continue the motions, termed its *diastole* and *systole*, a long time after the circulation has ceased, it is proved by this fact that these result from distinct powers of the heart, whether acting through the medium of the same, or of a different, structure; for the heart contracts when there is no blood to distend it, and its dilatation takes place, not by the force of the blood which operates in the absence of a contractile exertion, but by a faculty which then resides in the heart, and may continue for a while to reside in it when separated from all its animal connections.*

8. As the action of the heart continues after the circulation has ceased, we must refer the faculties by which it acts (if these faculties are obtained from another source) to the capacity which the heart possesses for a certain quantum

* Lord Bacon relates, in his Hist. Vitæ et Mortis, that he has seen the heart of a criminal, when torn from his body, leap up and down for some minutes, more than a foot high: and Mr. Boyle has seen the heart of an eel beat an hour in an exhausted receiver.

of the power by which it is animated; but if its vital offices are perfected in itself, and liable to be modified or suspended only by the changes of connected organs, then the actions which continue after the circulation has ceased are the last efforts of properties, the agency on which is commenced, but not finally accomplished; resembling the progressive effect, in the destruction of the offices of organs, which is produced by opium, or any other cause of affection terminating in death. We cannot conceive these motions of irritability, as they are termed, to proceed from a cause essentially different to that which maintains the circulation; nor can we conceive why the contractions, which sometimes take place with great force, should not be adequate to support the circulation, unless we suppose that the cause which finally induces the death of the heart, first occasions the cessation of the action of the arteries.

It may be mentioned in this place, that having occasion to kill an animal in a way which would prevent any irregularity of the circulation, I thought to have rendered the action of the heart at once extinct, by passing a stilet through the medulla spinalis: in this I did not immediately succeed, and rather than protract the pain of the animal, I removed the head, and immediately opened the chest. The

heart was beating vigorously; but the effusion of blood from the carotids had ceased; and as one of these vessels had been tied in a previous part of the experiment below the place of decapitation, the amount of the effusion was very trifling, so that a considerable quantity of blood must have been present in the vascular system at the time when the powers engaged in the circulation became extinct. A stilet was passed through the medulla spinalis: the contractions of the heart were not perceptibly affected. I suffered them to continue six or seven minutes, and then punctured the anterior ventricle with a lancet, in order to ascertain whether any blood was circulated through the heart; nothing escaped but a few red bubbles; the heart collapsed, and its action was suspended. The sides of the wound being approximated, the ventricle became again distended, as if with air; and a slight motion was resumed, which continued about two minutes.

As every physiological inquiry has for its most important end the development of the relations which obtain in the economy of the *human subject*, so it is necessary, in prosecuting the investigation, to confine the researches to animals which bear with the human subject a perfect analogy in the circumstances which form the topics of the inquiry. We cannot extend

the observations made upon cold-blooded animals to the phenomena of the mammalia, because the diversity of the laws to which they are obedient, is the proof of the absence of that analogy upon which alone the argument can be founded. Shall we conclude that the nervous system is not necessary to sensation, or the muscular structure essential to motion, because the zoophytes, according to Cuvier, display the faculties both of sense and motion, although they possess neither fibres nor nerves?*

We perceive by this analysis, that much has been attempted for the understanding of the mode of the action of the heart; that some of the particulars which have been investigated, are still undetermined; and that many more remain, which have not yet been submitted to the test of experimental inquiry.

* Anatomie Comparée, tom. 1. p. 27.

ARTICLE

ARTICLE IX.

Relation of Nerves with the Arteries.

THIS relation is to be considered in two points of view: the first respects the action of the arteries; and the second, the other processes of organic life, which are connected with the circulation. It is demonstrated that the phenomena of these two classes are, the first totally, and the second to a certain extent, independent of the central terminations of nerves.

That the action of the arteries of a limb is not dependent upon the medulla spinalis, is proved by the fact, that the pulsation of the arteries in the fore leg of an animal will continue after a division of the axillary plexus.

It is also demonstrated that the secretions, and the processes of regeneration, will likewise proceed below the place where the communication of nerves with their centre is intercepted. A testicle has been known to suppurate after the division of the spermatic nerves. But as I was

desirous of acquiring a more precise information on this point, than I had been able to possess by report, I divided, with as little injury as possible to the surrounding substances, the nerves of the axillary plexus of a rabbit, at a distance of about three fourths of an inch from the ribs: I was particularly careful to divide every filament of nerve; and as the operation was familiar to me, I believe that I succeeded. The limb lost its sensibility, and was rendered incapable of motion: the circulation was, however, maintained in it.

The wound, which was principally below the point at which the nerves were divided, went through the same stages as I had observed in the same animals, under other circumstances; that is, the skin sloughed to a trifling extent; the wound became covered with a whitish concretion, it granulated, and healed in less than a fortnight. At this period, the leg was diminished in size, and paralysed; but no remote destruction of integument had taken place, and the organic life was maintained. Finally, the animal recovered the use of this as well as of the other legs.

It is thus proved that the influence from a central termination of nerves is not necessary to the organic processes, which were in this

experiment observed to take place. I am acquainted with no instances in which this influence is in any degree indicated to be necessary to secretion, except the experiments of Mr. Brodie, before adverted to; in which it was found, that arsenic produced no secretion from the mucous membrane of the stomach and intestines, after a division of the eighth pair of nerves. On these experiments it must however be remarked, that if foreign agents are employed for the elucidation of a natural connection, the results, being liable to be modified by such agents, will not at all times be conclusive on the points to which they relate.

But although the brain does not appear to be in all instances necessary to secretion, yet the results of this process are variously influenced by the affections of the brain. Of this fact we have many examples: a passion of the mind is capable of influencing the secretion of the kidneys, as well as that of the intestines; and perhaps the liver itself is not exempt from the same kind of agency: the appetite for food, when stimulated by the *sight* of the objects of its desire, will increase the secretion of saliva. If we admit no conclusion, which is not supported by the testimony of direct fact and experience, we cannot pronounce that the influence of a nervous centre is necessary to any of

the processes of organic life. We have proofs that these processes are liable to be modified by certain conditions and affections of the brain; but that the dependence is in the relation of a source with a channel of distribution, we have no absolute proofs; our testimonies amount only to this evidence, that these are the effects (viz. those which have been noted from a sensible demonstration) of a division of nerves. Such appears to be the condition of the evidence which relates to the connection between remote organic life and the central termination of nerves.*

If the action of the arteries of a limb continue, notwithstanding the communication by nerves with the medulla spinalis is intercepted, it is next to be examined whether the nerves themselves hold, with respect to the arteries, a relation by which the phenomena of the latter are in any degree supported, or liable to be influenced. In order to proceed with this examination, it must first be ascertained whether the arteries possess a power of action (which is among the most conspicuous of their apparent phenomena) independent of the heart.

* This expression, "the central termination of nerves," is frequently employed; and I adopt it, after Reil, because the denomination appears to me good for the purposes of distinction.

Concerning the action of the arteries, there is still no trifling disagreement. The most common belief is, that their action (which gives the pulse) consists in an alternate contraction and dilatation of their parietes. There are those who maintain that the pulse of an artery is merely a loco-motion of the vessel: as this is a particular upon which the accumulation of facts is desirable, I hazard the following, which have been met with in the course of my own experiments and practice.

1. I had occasion to tie the carotid of a rabbit, with a view to the illustration of some suspicions which had occurred to me, with respect to the cerebral circulation. When this vessel was exposed for the space of $\frac{3}{4}$ of an inch, I did not immediately tie it, as there was in it no perceptible pulsation. I was particularly desirous to be accurate in this experiment, and therefore, for the purpose of ascertaining by another test the identity of the vessel, which was not manifested by its pulsation, I placed two ligatures under it, and made a puncture between them with a lancet; arterial blood was ejected to a considerable distance with jets resembling a tremulous propulsion: the want of a visible pulsation (to which my attention at this time had never been called) was noted. The explanation which occurred to me was, that

owing to the extension of the vessel, and the rapidity of the action of the heart, in consequence of the alarm of the animal, an uniform distention of the vessel was maintained. I could not believe but that the artery moved, either of itself, or by the motion of its fluids, when I recollected that I had often seen the skin of the neck alternately rise and fall, by the action of my own carotids; when I remembered, likewise, that I had seen a distinct movement of the brachial artery of a rabbit, which also pursues a straight direction, at a distance from an angle, in the course of my experiments upon the axillary plexus and the brachial nerves.

But at the same time as this exception was recollected, it also occurred to me, that the brachial artery was affected by the same causes, as those which suggested themselves as the circumstances of the above explanation: the fact, therefore, was remarked, while the explanation was held dubiously, or rather altogether rejected at this time, though afterwards in part adopted with different views.*

* This absence of visible pulsation has also been remarked by Dr. Parry. Dr. P. is convinced that an artery has no motion, except it is produced by pressure on the vessel, impeding the current of blood, and occasioning the sense of pulsation, in consequence of the impetus with which blood is projected against the finger. The same cause, or one in effect the same, is operative at the place of an angle in the artery, or where it

2. I exposed, for the space of an inch, the femoral artery of a rabbit: it was observed with the naked eye, and no motion was perceptible in it: it was examined with a powerful lens; a tremulous motion was observed in the vessel, which was imparted to the loose membranous substance which surrounded it. The rapidity of this tremulous motion appeared to correspond with that which was felt in it by the finger. This artery is removed from the influence of respiration.

3. The carotids of the same animal were in succession exposed for the space of an inch; they were separately examined with the following result: the vessel was raised and depressed, in the direction from the chest to the head, and from the head to the chest, by the actions of expiration and inspiration: the artery was examined with a lens, and it was clearly perceived that in the course of this elevation and depression it was actuated by two distinct impulses of its own, giving it the tremulous movement

passes over a muscle: Dr. P. has observed a motion in the carotids of a sheep, but it corresponded with the elevations and depressions which perform mechanical respiration. I cannot on many accounts agree in this explanation: but as I have reason to think, that Dr. Parry is engaged in an experimental investigation on the subject, I reserve the liberty of changing my present opinion, if, from an increase of facts, or a more philosophical arrangement of those we already possess, it should appear to be erroneous.

observed in the femoral artery. A ligature was placed upon each carotid in succession; no *visible* distention took place below the ligature, nor was the vessel actuated at this place by any motion which was not observable before the application of the ligature: the pulse is therefore not given by the dilatation of the vessel in consequence of the pressure of the finger, or of an impeded circulation.

4. As the blood is emitted from an artery with distinct jets, it is necessary to conclude that its impetus and pressure against the coats of the artery, are in a different degree, at different times; and we cannot but add, that as the coats of an artery are capable of distention and contraction, so it is necessary that an effect of this sort must arise out of the mode of circulation.

5. As the *volume of blood* is greater in a given space of an artery at one time, than at another, and as the calibre of an artery will correspond to a certain extent with the quantity of blood it contains; it is also on this account *necessary* to infer, that the calibre of the vessel is enlarged in a given space, when it contains the greater, and diminished when it contains the lesser, quantity of blood.

6. If the pulse of an artery were given only by the impetus with which blood is driven against the finger, it should be felt only by that side of the finger which is next the heart: whereas the pulsation of the carotid artery may be felt throughout the whole length of the finger when it is laid longitudinally upon the vessel.

7. If a sense of pulsation takes place in consequence of a partial impediment to the transmission of a current of blood, it should be perceived when the finger is applied to a vein, or when pressure is made against the urethra, during the passage of urine: but no such sensation arises from such pressure, though in the latter case, the impetus with which the fluid is projected, may be greater than that with which blood is thrown out from a divided artery.

8. From these premises it appears necessary to conclude, that the tremulous motion observable in arteries, is one of contraction and dilatation; that this motion is less distinct, when the action of the vascular system is preternaturally rapid (as under an experiment or painful operation), than in the usual state of health. The want of distinctness of pulsation under these circumstances may be expected to arise from the rapidity with which blood is thrown from

the heart into the arteries, tending to preserve almost an uniform distention. We find that the same thing happens in the course of practice: a pulse of 70 or 65 is commonly more full, and distinct than one of 90: a pulse that beats at the rate of 160, is small and confused; if the action is more rapid than this, the number of pulsations cannot be precisely counted.*

9. Unless we admit that the tremulous motion of an artery is one of contraction and dilatation, we possess only one other alternative, which is a consonance with the facts, from which it is necessary to infer this kind of action, (viz.) that the dilatation and contraction of an artery affect only the internal and middle coats; and that this action is felt, when the external is approximated to the middle coat, by the pressure of the finger: but such a conjecture is not supported by any examples, and therefore does not appear worthy of consideration. That the arteries not only contract and dilate when engaged in the offices of the circulation, but that

* I have heard it affirmed that a pulse at 160 in a minute admits of being enumerated: I cannot however affirm this from my own experience. In the last stage of phthisis, a pulse which appeared to me about 140 in a minute, has in truth afforded no distinct pulsation: the radial artery has appeared to tremble under the finger, and its action has reminded me of those *visible* motions, displayed by an artery when laid bare in an experiment.

they possess this faculty independent of the heart, appears from the following circumstances.

1. The pulse in the radical artery has been known not merely to remit, but to stop totally, and at the expiration of a considerable interval of time (as of some hours, or days) to be resumed; while the pulse in the other arteries has been distinctly continued.* As the influence of the action of the heart, whatever this action might be, cannot alter the relation which the blood, present in the aorta, holds with the first branches which proceed from it; so the cause of this irregularity must be looked for, not in the action of the heart, but in the condition of the primordial trunks. This condition will be comprehended in two alternatives, either of which, proving a distinct power of action in the arteries, will be sufficient for the purposes of the citation.

These alternatives are, 1st, Either that the other arteries which proceed from the aorta have transmitted all the blood which was contained in this trunk, in consequence of a condition

* This is a circumstance which has been remarked as not unfrequent in nervous and hysterical affections: I have also known the pulse to be entirely wanting in both wrists, while it has continued in the carotids, for some hours preceding death, in a case of enteritis.

which they had assumed, and in which the artery, the pulse of which was deficient, had not participated; or, 2d, That this last vessel had adopted a state of rigid contraction, which was either total, or partial and uniform; the first preventing the entrance of blood, the second precluding the dilatation of its parietes by an irregular impetus of blood. The first of these alternatives is in consonance with the most frequent analogies; and the peculiarity will be comprised in a disproportionate action, or in a disproportionate calibre. Of the second alternative I shall say nothing.

2. It is proved by Haller, that an artery will empty itself of its blood in its remote distribution, after the application of a ligature;* which it cannot do from the impetus of blood, driven from the heart through its own channel.

3. The branches of the pudic artery (the dorsalis penis, &c.) may beat a hundred strokes in a minute, while the radial artery, though perhaps affected in some degree by the same causes, may be found to beat only at the rate of ninety.

4. If the heart acted with the astonishing

* Haller mentions that the evacuation of blood takes place from the portion of an artery which is included between two ligatures.

force which Borelli has assigned, it might perhaps be adequate to produce a circulation throughout the vascular system, by a vis a tergo: but as the blood from a half divided aorta, or carotid, in the largest animals, either of which is very near the heart, is not projected to a greater distance at most than three or four feet; we cannot conceive this projection to be equal to perform the circulation through the whole arterial system, through the secerning extremities, through the lymphatics, through the veins, and their numerous channels from the remotest parts, under every variety of position, back to the heart. But as the calculation of Borelli did not strictly refer to the force of the action of the heart in a single systole, and as the computation of Keil was founded upon the actual velocity of the circulation; so, if we accept either calculation, it seems most reasonable to prefer the latter. The force of the heart assigned by Borelli, as is well known, is equal to a weight of three thousand pounds; as assigned by Keil, equal to the weight of eight pounds.

5. In a violent inflammation (terminating in abscess) of the foot, I have found the anterior tibial artery where it passes over the tarsus, beating regularly twenty-three strokes in a quarter of a minute, when the radial artery has beat only twenty-one.

6. The dorsalis pollicis in my right hand is large and conspicuous, so that the action of it may be seen as well as felt. I have frequently produced an inflammation of the skin covering this vessel by external irritation, and I have felt it beat twenty-three strokes in a quarter of a minute, while the radial artery has beat only twenty-one in the same time. I have known the increase of frequency greater than this. But the accuracy of this experiment is not perfectly unexceptionable; as the examination of the two arteries was made in succession, and the means, (such as friction &c.) employed in irritating the dorsalis pollicis, may produce an increase in the action of the heart, which would have subsided or diminished by the time that the examination of the radial artery was made.*

We do not however stand in need of this test: one proof is sufficient to establish the fact of a power of action in the arteries, independent of the heart; and this proof is furnished by any irregularity of the circulation in the different

* I have since made the following experiment, with the same vessel:—The pulsations of the arteries generally, were at seventy; the dorsalis pollicis has been irritated, it has beat eighty-four strokes in a minute; the radial artery was immediately examined, and was found to beat seventy-two strokes in a minute; the examination was again transferred to the dorsalis pollicis, the pulsations of which still continued at the rate of eighty-four in a minute, the vessel being visibly enlarged by the friction.

parts of the sanguineous system; or by any authentic fact, that the blood is propelled by the contraction of an artery, without the aid of a vis a tergo.

I have been publicly informed that a vessel may propel its fluids, without either a pulsatory action or the aid of a vis a tergo; and it has been cited in illustration, that the veins empty themselves of their blood after a ligature has been applied, as is exemplified in phlebotomy &c. which would not be the case if the blood were circulated in the veins by a vis a tergo, and which is not accomplished by the possession of a faculty of pulsation. But that the veins do always immediately empty themselves above a ligature, is not the fact.

Haller mentions that if a vein near the heart is tied, the superior part of it becomes pale and flaccid. We are to consider in this example, that the right cavities of the heart have a capacity to admit more blood than is sent to them, and that there is but little resistance to the entrance of blood from the vein. If the ascending cava were tied near the heart, its blood would be carried on to this organ, not by the pulsation, or by the immediate contraction, of the vein, or by a vis a tergo, *but by the impetus with which the blood was before endowed.* The

effect might take place in this way without the co-operation of muscular fibres (said to be possessed by this vein) which are but faintly marked; and which are sometimes observable, where there is no consequent action (as in the muscles of the external ear) and which at other times are not to be discovered, where the actions take place for which muscular structure is supposed necessary; as exemplified in the motions of the iris, and in the actions which regulate in some degree the focal powers of the eye, whether by affecting the sphericity of the cornea or that of the chrystaline lens.

But the evacuation of blood from a vein does not take place under similar circumstances throughout the venous system; for if a vein (as one on the back of the hand when they are all distended by exercise) be compressed at one point, it will remain turgid both *above* and below the place of pressure. This effect does not take place in the same way in different veins. If there be a valve between the point of obstruction and the next superior communication, the blood which belongs to the superior portion of the vein will not be discharged. If this blood be pressed out, the superior part of the vein is neither visible, nor to be rendered visible by retrograde pressure in the course of the vein, between the valve and the place of obstruction. In this case the

blood must either coagulate in the vein, or be gradually compressed out, by the contraction, which takes place remotely, of the vascular parietes. Whether this is the effect of the contractility of tissue, or of an operation of the agent of life, in consonance with some general law, we are scarcely informed.

But if no valve be interposed between the point of pressure and the next superior communication, the blood in the vein above the obstruction may be discharged by pressure, and the vein will fill again: it is probable therefore that a circulation is maintained in the superior part of a tied vein, as low as the place of a valve, the blood being obtained by a reflux from superior communications.

As the vein receives blood above the ligature from other branches, the circulation will be continued in the vein with the motion with which this blood is actuated: in the veins, the current is uniform. Nor is the fact adduced by Haller demonstrative in every case of an independent power of action in the arteries, if a state of collapse did not intervene between the application of the ligature and the renewal of the circulation; for the same law holds good in regard to them, as that which we have noticed with respect to the veins. The tied artery receives

blood by communication with unobstructed arteries, and the blood is continued in the remote parts of the artery with an irregularity of propulsion which corresponds with the mode in which its blood is obtained. Haller however mentions that the *aorta* will empty itself below a ligature: and I do not perceive that the above exception can apply to this instance.

The circulation will be retrograde for a short space in the vein, under the circumstances last described; it will also be propulsive in the same part of the tube. This is a state of imperfect circulation of the blood, which tends to coagulation: the same kind of circulation obtains for a time immediately above the ligature on an artery; but this circulation is not long maintained, the blood in either example finally coagulates.* These circumstances indicate very strongly, that the phenomena of the circulation in the veins are purely mechanical.

* I am inclined to think that there is a property of preserving for a certain time the fluidity of the blood, belonging to the vascular system, independent of motion; and that this property is one dependent upon the nerves. The first part of the position is in a great measure confirmed by the formation of a clot in the *sheath* of a divided artery, which would no more take place in the sheath than in the vessel itself, if the fluidity of the blood were wholly dependent upon its motion; for this motion continues while the coagulation is taking place. The coagulation of the blood is in this instance accounted for by supposing that it becomes entangled &c.; which is a very entangled sort of a supposition, because it

The blood drawn from a vein is at first dark: it may assume the colour of arterial blood, when 8 or 10 ounces have been abstracted; or it may be emitted with a faint jet: both these circumstances arise from the facility of the transmission of blood from the arterial, into the venous, system, which is afforded by the diminution of the resisting volume. When the transmission of blood from the arteries into the veins is facilitated, as it is in this way, the circulation between the two systems is accelerated; and sufficient time is not suffered to elapse for the conversion of arterial, into venous, blood, which is a function of the capillaries, though not understood; hence the sensible resemblance of the blood emitted from a vein, to that in the arteries.

The removal of the same cause, (viz.) the efficient resistance of the column to be circulated by a vis a tergo, admits a freedom of influx between the arteries and the veins, by which the blood in the latter would be actuated by the same motion, as that in the former. A dimi-

flows in a space, and through this same space it may continue to flow, if its fluidity were maintained by its motion. This proof is not perfect, because the fact is not unquestionable; at least, the assigned order of its occurrence is not unquestionable. With regard to the second part of the position, the reasons which affect it are mentioned elsewhere.

nished power of propulsion, which obtains in the small arterial branches,* and an increased resistance of the blood in the veins arising from the absence of an active power possessed by their coats, concur to prevent pulsation in the veins; and this co-operation is sufficient, without the aid of any other arrangement. When this resistance is removed, under circumstances which may be defined, but which need not here be entered into, the emission of blood from a vein will take place with a faint jet. Mr. Hunter remarks that this is a common circumstance when we bleed in the hand or foot; and observes upon it, that in fact the veins have a faculty of pulsation.†

There are other conditions of the vascular system which may determine the same effect; but they must operate virtually by the same mode. One of these other conditions consists in a dilatation of the minute arteries, communicating with the veins; by which the transmission of blood from the arterial, into the venous, system, would also be facilitated. Such a state of dilatation is one which the arteries are much

* This fact is evinced by the greater velocity with which blood is thrown from a wound of an artery which immediately proceeds from a trunk, than from a branch of the same size, which is given off as a distant ramification.

† Treatise on the Blood, p. 186.

disposed to assume: it would convert capillary extremities into vessels of a larger order, and produce the same motion of the blood in a vein, as if it were sent into it from a considerable arterial branch by continuity of tube.

A remarkable case has fallen under my notice, in which the jet of blood from a vein was conspicuous: the blood was also of a bright scarlet colour, and apparently as fluid as water. The case was as follows.

A woman flooded after delivery almost to death: the placenta, which adhered very firmly to the uterine parietes, was extracted; and there was scarcely a sensible testimony of the continuance of life. The action of the heart was perceptible, but more than a quarter of an hour elapsed before the pulse could be distinctly felt at the wrist. Vomiting supervened upon the exhibition of a dose of laudanum, which was followed by the contraction of the uterus, and then the flooding ceased. The woman lay about half an hour with scarce any signs of life. The pulse during this time was rising in fulness and in frequency; and at the end of the half hour the pulsations, which were just sufficiently distinct to admit of being numbered, were about 50 in a minute.

The woman was got into bed; she recovered rapidly, and in two hours her pulse rose to upwards of ninety; it continued afterwards within the range of from 90 to 110; and in twelve hours from the cessation of the hemorrhage it presented the sensation of a plethora which could not be exceeded. This circumstance (which is not an uncommon one) arose from the disproportion which was occasioned between the blood as a resisting, and the circulating organs, as an active, power.*

On the 10th day from her delivery, being as well as women usually are at this period, the medical superintendence ceased, while directions for the continuance of a low diet were enjoined: these directions however were not complied with; spirits were given her by her nominal friends, and on the eleventh day she made a hearty dinner on roast beef and strong beer.

She slept well, had no fever, the bowels

* It is commonly believed that the abstraction of blood diminishes the action of the arteries, by reducing the quantity of the pabulum from which the principle is renewed: how then did it happen in the above case, that the reduction of arterial action was not proportionate to the loss of blood? Whatever the nature of the connection between the blood and the properties which animate the circulating organs might be, it is evident that the natural force of the latter, in the present instance, was not diminished.

were regular, the uterine discharge natural, the tongue clean, and the secretion of milk copious, until the twelfth day; when she had two or three rigors: in the night she became delirious, and on the 14th day she died. Her tongue remained clean, the discharge from the uterus had terminated naturally, and she felt no pain either spontaneous, or excitable by pressure, in any part of the body, up to the hour of her death. After the rigors had taken place, the pulse ranged between 110 and 130 in a minute; bounding, and apparently involving the fullest possible distention of the vessel.

It was this state of the pulse which indicated the propriety of bleeding: the blood (taken from the median cephalic) was, as I have described it above, thin and of a bright colour, looking like water tinged with vermilion; and it was emitted to a considerable distance with regular undulations. The depletion was speedily put an end to, when this appearance of the blood was observed. To go into all the remarks and inquiries suggested by this case would fill a volume, and therefore they cannot here be particularized: nor is it necessary, for the purpose of obviating objections to that pulsation which we have affirmed to obtain in the arteries, to extend further the discussion respecting the veins.

As it appears to me to be proved by the facts which have been enumerated, that the arteries not only have a movement, but that this movement is one of alternate contraction and dilatation; and further, that this pulsation is not merely the effect of the force with which blood is thrown into the arteries by the heart, as the only agent of circulation, but results from a power of acting inherent in their own structure, which in health generally agrees with the diastole and systole of the heart, but which under disease is liable to deviate from this correspondence; since all this appears to me to be established beyond controversy, it is next to be inquired, what is the origin of this power of action, which has been affirmed to be inherent in the arteries? We cannot at once inquire after the source of this power of action, without first bestowing a consideration upon the property itself; and without examining how far it is connected with some agent which will be recognized as a more general principle.

As the circulation may cease in a part which is dead before the decomposition of the textures has taken place, we must refer this cessation of the circulation (which involves the extinction of the pulse in the arteries) to the cessation of the principle of life. As the diffused presence of this principle of life is necessary to the

continuance of the actions of the arteries, we are thus furnished with an additional proof, as well of the inadequacy of the heart to maintain all the phenomena which take place in the distributions of its own system, as of the necessity of an influence remotely conferred, to preserve these same phenomena. We have examples of the fact, in the instances of amputation for gangrene; in which, the circulation is found to be stopped at a place where the integrity of the textures appears perfect, and at a distance above the sensible line of mortification. We have many similiar examples, some of which may be more striking; but as they are familiar, they need not be detailed.

How, it will be inquired, is this cessation of the action in the arteries accomplished? What is the process by which it takes place? The immediate cause of the want of circulation will be found in the coagulation of the blood. It will be observed that this effect does not invariably take place upon the extinction of life, for the blood is sometimes found fluid after death. But the coagulation of the blood takes place generally where life is extinct: and its fluidity is preserved, notwithstanding its own tendency to coagulate, where life is present. It therefore appears that the regulation of this particular is dependent upon the states of the principle of

life; and the more especially, as chemistry has detected no varieties which account for the diversity of the effects, upon chemical principles; and as we observe that the central organ of circulation, the heart itself, is dependent upon the same agent.

If a mechanical cause of obstruction were assigned, such as that which was supposed in the old doctrine of the error loci, &c. it would be found upon examination that this cause is also inadequate; for the obstruction is supposed to commence in the minutest capillary tubes, an order of vessels, which do not naturally carry red blood: the circulation *has no dependence* on these vessels, while those upon which it does depend, namely, vessels capable of propelling and returning freely red blood, are pervious, and still adapted to their function.

We are also well assured of the tendency of abstracted blood: it coagulates when placed beyond the influence of the principle of life; nor will it remain fluid in the body after death, unless previously impressed and affected by the condition which the principle had assumed, and which was preparatory to its final extinction. I look for no further evidence on this point, because that which I have adduced appears to me conclusive: others may not admit the in-

ference; but before they decide against it, I would request them to consider the question for themselves, that is, both sides of it, and they will perhaps discover proofs of the affirmative, which are to them more striking and satisfactory. They may otherwise discover testimonies of the fallacy of the argument; of these I should feel happy to be instructed. I will set this point down, therefore, for no petitio principii, but will assume the position as one, the legitimate title to which has been made sufficiently clear.

The remote action of the arteries then requires the presence of a principle of life: but this principle, displaying many agencies, is not, as has been before remarked, an homogenous, simple, elementary principle: its properties are greatly diversified; and they are conspicuous wherever they are exerted, that is, in every occurrence which belongs to a living body.

The properties of the principle, as is clear from the different orders of structure which are subservient to them, have their distinct alliances with the parts of texture: in a muscle, it displays all the faculties of contraction except that of tissue: in the arteries, it maintains their action, and maintains the fluidity of the blood; perhaps with the concurrence of motion, which

also results from a previous act of the principle, and furnishes another example of the harmony with which this spirit exerts itself in all its provinces; for, as before remarked, motion and temperature are not sufficient to preserve the fluidity of the blood. In the secerning extremities, it is again a modified principle of life; or, in other words, it displays another property: here, it has for its subject the same constitution, namely, the blood, as that upon which it elsewhere operates; it elaborates the secretions, by an affinity of identity, with the fluids: for the laws of causation which affect this question, must be universal; and it may be evinced by testimonies, to which we cannot refuse our assent, that they must be inferred, in examples which are not seen.

This, it will be said, is an easy enumeration of many intricate, and perhaps doubtful relations; but we will recapitulate the topics, and append to each of them the evidence, which at least appears to afford a satisfactory support.

1. That it is a property of the principle of life (which must always be considered specifically, in reference to the subject in which it is contemplated) which is connected with the muscular structures, and affords their various powers of contraction, appears from this fact,

that the cessation of the organic, involves that also of the animal life: the latter therefore does not exist per se, but is connected in the relation of dependence upon the former: this relation, however, is not mutual; as the organic life may continue, though not I believe eventually without slight affection, where the animal phenomena have ceased. This connection justifies us in considering the properties which belong to these two departments as parts of a general principle, which are united for an end to which their concurrence is essential, namely, that of constituting an *animal* body.

2. We must next speak of the secerning extremities, considering that the question relative to the motions of the arteries, and the fluidity of the blood, has been already discussed. That the secretions take place in the way which has been assigned, seems to be indicated with some force by the following facts: 1st. If a limb, or an organ be dead, notwithstanding the integrity of the textures is preserved, the secretion (of which we might select any example indifferently) will not be produced, or any thing like it, as in the way of filtration of blood through dead tubes. 2d. If the circulation be precluded, or the blood improperly constituted as by mixture, or if another fluid be substituted for blood, the secretion will be modified by the first, as long as life

continues, and by the last it will be incapable of taking place. It appears fair therefore to deduce, that the secretions are also results of the same principle of life; and that the relation of this principle, in reference to such purposes, is with the fluids of a living body. The end is thus shewn to have a double dependence, and the fluids and the principle a reciprocal function.

Another part of the position which has been affirmed, is not the least essential, namely, that the principle operates by its identity. That it does not operate by an impulse given to the fluids, appears clear for the following reasons: 1st, That an impulse respects an action whose only diversity is that of degree; 2d, That all the degrees of the same action, which must be propulsive on the fluids, may be imitated: the fluids employed, may be from the subject of the experiment, or another of similar constitution; and if the principle be absent, the secretion will not be produced.

3. That the secretion is no chemical result, independent of the properties of the principle, is evident from this fact, namely, that the same fluids will not furnish the same secretion, under circumstances in all respects similar, except that life has become extinct.

4. That the identity of the principle governs the secretion, as the chemical constituents will effect a product, is evinced by the necessity of a precise constitution, similar in this respect to that of the blood; for, without this identity, the secretion cannot be made to appear: this fact is illustrated more or less strikingly in every history of disease. Our experience furnishes us with no examples of an agency which does not take place by mechanical motion, or by the presence of properties: the last kind of agency might, perhaps, involve a corpuscular movement of parts, for, without it, it is difficult to conceive the possibility of change; but it is not of that perceptible kind, which results from the common acts of gravity. It involves an operation, however, which must take place as well in things which we know to exist by their effects, but which we cannot see, as in those which are cognizable to the senses. In this point of view, the definition, though perhaps arbitrary, appears proper for the purposes of distinction; but as far as it affects this argument, it is of no importance. The mode of action which I have said takes place by identity, is one which has been accepted by the chemists; and the act itself is designated by them one of affinity, one dependent upon properties, distinguished from those which are common in some degree to all matter. If we inquire further,

with a view of defining an ultimate difference in these modes of causation, we shall perhaps arrive at an unhoped-for conclusion; or we may find that we stand in need of another sense, in order to make any conclusion on the subject: the want of this sense is often experienced by those who deal in subtilties.

It has been affirmed by the chemists, that the blood does not contain the secretions: their inability to discover them only proves that their laboratories furnish no identity of life; that they do not possess the agent, either by combination with which the matter of secretion may be produced, or by the influence of which components may be separated in the blood, new combinations formed, and products eliminated, which, for a very natural reason, elude the coarser tests of imitation.

It has been remarked, that these properties, so far as they are known, affect respectively some order of structure: as we have seen the necessity of the existence of the principle of life remote from the centres of the nervous system, it is next to be inquired what is the source of this principle, or whether it acknowledges any source, as in the way of derivation?

Notwithstanding the action of the heart, and the integrity of the brain and spinal mar-

row, may continue, life might elsewhere become locally extinct: and although the communication of the brain and medulla spinalis with remote parts should be intercepted, the cessation of phenomena is limited to the faculties of sense and motion; while a vitality is preserved, which will maintain the cohesion of the textures, and perform the offices which are enumerated under the department of the organic life.* This latter, therefore, we find to have no direct dependence upon the *nervous centres:* it remains to be inquired whether it is equally independent of the nerves, whether it is exclusively conferred by the presence of arterial blood? This point is one, the decision of which is difficult: I shall, however, offer some evidence which appears to influence the question.

I had occasion to tie the nerves of the axillary plexus of a rabbit:† three or four ligatures were employed for this purpose, and all the nerves of the plexus, which have an inferior distribution, were in this way intercepted. The integument of the foot first sphacelated, and

* Of this fact a satisfactory example has been before adduced, in the experiment of dividing the axillary plexus, after which the wound suppurated and healed; the limb lived, and finally its animal capacities were restored by reunion of the nerves.

† This experiment, which was made for another purpose, will be subsequently detailed.

the process was extended to within about two inches of the place of the injury of the nerves. In this way the bones became perfectly exposed, without occasioning to the animal the slightest apparent inconvenience. The death of the limb was complete to the above mentioned extent, and all the nerves were found to be totally destroyed below the ligatures, except one, which was the smallest of the plexus, and reached as far as the line of distinction between the dead and the living parts. This result has many analogies in the cases of surgery; but the specific action upon the nerves is not in them so well marked. The following questions arise out of the coincidence exhibited in the experiment.

1st, What was the remote cause of the death of the limb? It must be replied, the injury of the the nerves. 2d, In what did this injury consist? This question is one which must be discussed.

As the central influence was prevented, it is necessary to conclude that actions may be carried on by remote nerves, which actions, as the same cause would not produce them in the dead subject, must be founded upon a natural function of these parts. As the end of this modification was the extinction of the organic life, it is thus included in the facts themselves, that the organic life may cease in consequence of a change in

the function of *the nerves.* This distinction alone is illustrative of many questions of pathology.

If the injury of the nerves is said to have been operative, first, throughout the extent of their own structure, and that the death of the other orders of structure followed in consequence, it is thus shewn that there is no other order of structure capable of maintaining life, when the function of the nerves is destroyed. But if the arteries conferred the life which exists in remote parts, why was it not maintained in the nerves? The modifications of *their* function have no reference to death, if life be exclusively preserved by the presence of blood. A part which is dependent upon another for life, having no other importance than that of a passive and subordinate instrument, should live, if the perfection of its source is unimpaired.

That the nerves did not die from privation of central influence, is shown by the fact, that the inferior parts of nerves commonly live after their division: it is true, they sometimes die; which, as it is not a necessary effect, must be referred to the operation of the injury upon a peculiar state of the nerves, disposing to unusual events.

The arteries have no power of maintaining life independent of the nerves; we say therefore

that life depends upon the presence of blood, because where this latter is not, there is no life.* The presence of the blood then, and the function of remote nerves, are equally essential to the preservation of life; for death follows where either is prevented. If there is any argument which throws a doubt upon this conclusion, it is one which has been before said to interfere very much with our inferences, and to make us conclude with modesty on many other occasions. The argument to which I allude, is that in which two modes of causation are designated, one by privation, and the other by communication of influence. But if we reject, on this ground, that kind of agency which has been attributed to the nerves, independent of their centres, it is equally applicable to the importance and operation (namely, of conferring life) which are otherwise assigned to the presence of arterial blood: a parity of reasoning may be shown to obtain in all respects.

* The tendons, &c. will be said to be exceptions: but all parts, though permeated by fluids, do not require *red blood;* they require a fluid which is adapted to their proper growth and nutrition; this they obtain, which performs with regard to them the offices of red blood. Reil believed the influence of the nerves to be diffused further than the presence of the material organs; I am disposed to acquiesce in this opinion: and it is probable that the vitality which exists in tendons during life might be quoted as an example.

If this matter be minutely scrutinized, we shall find that very little of our knowledge has a more unexceptionable foundation, than the law in which I am inclined to acquiesce from the force of the facts which are just related. The law to which I advert, is comprehended in the discussion: it is this, that the nerves may live independent of their centres; that they cannot live independent of the arteries, any more than the arteries are capable of living under a destruction of the offices of the nerves: the fate of the other textures is conformable to that of the arteries and the nerves: life exists where these are, it cannot be destroyed but by an operation upon them; by their injury it is modified or ceases, by their co-operation it is preserved.

But the principle is not conferred by the presence of blood, for if it were, transfusion of arterial blood should re-animate an extremity of one recently dead. The presence of a diffused principle of life is necessary to maintain itself, while, as it is expended in actions, it requires the presence of a matter of assimilation. As the arteries distribute the latter, and as the function of remote nerves is necessary to the purpose of such distribution, that is to the existence of life; so it is necessary to conclude that the maintainance of the principle is a con-

joined result; that the organic spirit is allied with the remote nerves; that it assimilates from the blood an agent which chemistry, boasted chemistry, has never been able to detect in it, namely, its own identity. This process of assimilation is one which furnishes a vast deal of interesting speculation: but as such speculation would be liable to proceed without the sanction of direct facts, I forbear to prosecute any suggestions on the subject in this place.

Conformably with the result of the preceding discussion, it is to be believed that life, or the point of assimilation, and afterwards the source of vital actions, is allied with the structure of the nerves. It is to this agent that we refer the phenomena which are also connected with the arteries, and their terminations: it is to the influence of this principle, operating by its identity, rather in the mode of chemistry than that of mechanics, rather by affinities and combinations, than by perceptible movements, that we refer the government of the arteries, and all the processes of secretion: this reference is made to the principle as a concurring and essential, but not as an exclusive agent. Its modes of change (for it may be vastly modified without its extinction) are by privation of influence, and by communication of influence; which last I sometimes denominate affection.

If these conclusions (the premises of which I have mentioned, and they are therefore open to refutation) are correct, we should find them to be not only consistent with the arrangements of structure and the laws of function, but to agree also with the phenomena of disease: in order to ascertain this correspondence, we will apply them briefly to one example, selecting that of inflammation as the one which furnishes the most varied illustration. The enumeration of processes may not be in the order of occurrence, but a mistake of this sort will not affect the argument.

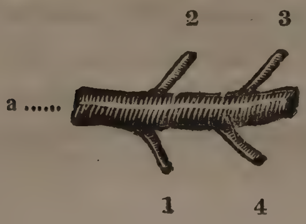
The series of actions which constitute inflammation may be arranged under two classes: namely, primitive acts, which refer to the affection or change of the principle of life; secondary acts, which refer to the government of the chemical constituents and the condition of the structures: the last are hydraulic and mechanical: these acts are interchanged, and are capable of influencing each other.

The symptoms of inflammation (as they are seen, rather than as they are expressed) are,

1st, Redness and swelling of the part in which it takes place. As vessels carry red blood in the state of inflammation, which in the natu-

ral state do not carry red blood, it is necessary to conclude that the quantity of red blood in such part is preternaturally increased; which in some measure also accounts for the tumefaction. This increase of the quantity of red blood may be obtained in two ways, both of them operating by a *present disproportion;* namely, by an accelerated action on the part of the branches which occupy the seat of inflammation, and by an increase of caliber in the same vessels: these are liable to be complicated.

1. That an accelerated action may take place in the arterial branches, independent of the heart, or their trunks, is proved by the examples before mentioned, in which this accelerated action was perceived. It will be replied, if the trunk is uninflamed, or beats in its natural time, the branches can obtain, and can therefore circulate, no more blood than is transmitted by a trunk in its usual condition, and the quantity of blood cannot, therefore, be increased. This argument is, however, entirely fallacious, as may be shown by a simple diagram, selected from an analogy in the subject of our considerations.



The blood flows through (a), which is an arterial trunk, with a natural velocity; the inflamed branches (3, 4) empty themselves, in consequence of an accelerated action, faster than the uninflamed branches (1, 2): there are then times when there is no resistance of a remoter volume at (3, 4), while there is the resistance of a remoter volume at (1, 2). The effect of this relative condition is, that the branches (3, 4) being capacitated, from the state of their action, to transmit more blood than (1, 2), do absolutely obtain it from the trunk, in consonance with this law of hydraulics, which must be applicable to the circulation (though all the laws of hydraulics are not), namely, that the greatest quantity of a fluid takes its course where there is the least resistance.

That this capacity of inflamed arterial branches to transmit an increased quantity of blood, depends upon a disproportionate condition of them, is proved by the fact just mentioned, and by the additional one, that no inflammation will take place from this accelerated action alone, when all the arteries are beating at the rate of 130 strokes in a minute; which may exceed, by ten or twenty, the pulsations of an inflamed branch which take place in the same time. It must also be acknowledged that, in many of the examples of inflammation, it may

perhaps be said, generally, the disproportionate action does not obtain.*

2. The increase of caliber in the inflamed branches is generally, if not always, conspicuous.† The digital arteries, which can scarcely be felt, when not enlarged, through the thick skin of the finger of a labourer, may in the same subject be found to beat with the fulness of the temporal artery, when the finger is affected by a whitlow. This increase of caliber might be assumed as an effect of disease, or in consonance with a natural function: of the former an example has been just cited; of the latter we have an illustration in the branches of the pudic artery, and in those of the uterus. The dorsalis penis, whatever the condition of the integument might be, whether rendered tense or corrugated, may be but just perceptible; in another condition of the organ, the distinct-

* I have remarked it only in two cases of inflammation; one of them has been already mentioned, the other was as follows: A woman, when rubbing a deal table, forced a splinter of wood half an inch in length under the nail of the second finger; it produced intolerable pain and rapid swelling: about three quarters of an hour after the accident I extracted the splinter, and, upon examination, found the arteries which supplied this finger beating 104 strokes in a minute, while the radial artery was beating only 92.

† Upon this principle, cold lotions, tending to restrict vascular dilatation, may check inflammation, by a modus operandi which is an adjunct to the reduction of temperature by evaporation.

ness of the pulsations, and the fulness of the vessel, are greatly increased. That this simple increase of the caliber of the artery, without any accelerated action, may lead to the accumulation of blood, and the distention of the cells of the organ to which (with others) it is distributed, is evinced by the fact, that when this accumulation of blood is taking place, or has occurred, the artery will sometimes be found to beat in the same time as that at the wrist. In the other example, namely, in that of the uterine arteries, it is needless to advert to the difference of their size in the unimpregnated state of the uterus, and that in the last months of utero-gestation.*

In addition to these remarks on the caliber of arteries, it is right to observe, that the external impression of the pulse of an artery (unless indeed this is nicely discriminated) is not at all times a criterion of the volume of blood it circulates; for this pulsation may be strong and bounding, at a place of the artery which is perfectly emptied at each contraction: this was the kind of action which obtained in the before mentioned case of flooding, where the volume of blood in the vascular system was in fact greatly diminished. It is evident that this total

* These instances of the dilatations of arteries will not be referred to debility, or expressed by any so absurd an appellation.

evacuation of blood from a given space of an artery, must be productive of the transmission of a lesser quantity of blood, than when the artery acts in the same time, but, owing to a real plethora, transmits its blood also in a *continued stream*.

As these dispositions of an artery, namely, to alter its caliber, or to accelerate its actions, are not manifested when the living principle is extinct, we cannot but refer these phenomena to that principle of life without which they do not take place, without which they cannot take place, though all other relations should be preserved. If this principle be allied with the nerves, and constitutes their function, we may then say that these actions result from an influence on the nerves, if we prefer assigning them to a material, rather than to a less comprehensible agent.

The same principle not only regulates the caliber, but the cohesion also of vessels: in acute inflammation, that property of the principle which prevents rupture of the arteries is generally increased, as the vessels in this state bear a great degree of distention: we have however an analogy for this in the arteries of the penis; we may therefore presume that the natural strength of the property is not diminished in

inflammation. In the local determination of blood to which apoplexy succeeds, the force of this property must be diminished; because I have seen a man, who had had two attacks of apoplexy, sustain phrenitis without such effect. This distinction, which has been deduced from my own experience, is very similar to one of Mr. Hunter, who supposed that in phlegmonous inflammation there is an increase both of the action and of the power of the vessels; while in inflammation which terminates in mortification there is an increase of action, but not a corresponding increase of power:* by this word "power" (than which no expression can be more vague), I understand the property of the organic spirit, by which the cohesion of the fibres of vessels is preserved.

As the remarks which have fallen under this title are already more extended than is conformable with the plan I proposed, I shall proceed to enumerate briefly the particulars of the application for which this subject of inflammation was introduced. We have spoken of the increased volume of blood which exists in an inflamed part: it remains to trace and to connect the processes of some other phenomena, which are also conspicuous.

* Treatise on the Blood, p. 8.

2d. Tumefaction; from the presence of an increased volume of blood, with perhaps effusion, arising from a complication of the hydraulic with the nervous operations: that is, the secretory extremities assume the same increase of caliber; and their secretion is dependent upon the relation of the fluids they contain, with the *identity* which the influence of the nerves has adopted.

3d. Pulsation of the inflamed part; from the dilatation of the minute arteries, which enables them to carry as much blood as is natural to a larger order of vessels, converting them from capillary tubes into arteries capable of independent action.

4th. This increase of caliber in the minute branches would be another cause of vacuum at many places, and the demand for blood from the trunk is thus augmented; tending also to increase the volume of blood in the inflamed part, and even to increase the caliber of the supplying branches.

5th. Redness of inflamed parts, from the assumption of the office of arteries by the capillary tubes.

6th. In all the affections of the vascular system, influencing the circulation, there must be a

balance between the office of the arteries and that of the veins; for the arteries, whatever may be the comparative distensibility of the two orders of vessels, must cease to act when the blood ceases to flow; and the influx of blood must be proportioned to the capacity of the veins for accomplishing its return to the heart: this fact is also exemplified in the phenomena of the branches of the pudic arteries. This arrangement tends to limit the volume of blood circulated by the inflamed arteries; and when it continues to obtain, until the modified function of the nerves is restored to the natural function, the inflammation may terminate in resolution; but if the modification of the function of the nerves, or, in other words, of the cause of inflammation, should go on to increase, the extreme arteries may obtain blood which they cannot transmit; and this blood must coagulate, unless the state of the cause of inflammation should, before this happens, operate to the destruction of the textures.

These effects principally comprise the results of an hydraulic agency, which are however irregular in the different examples of inflammation; the whole process acknowledging a primitive dependence upon the state which the function of the nerves has assumed, the mechanical condition being only to a certain extent a concurring agent.

The direct results of that cause of inflammation by which its actions are commenced are:

1st. The government of the caliber and action of vessels.

2d. The maintainance or destruction of their eohesion.

3d. The communication of an influence affecting the secretions.

4th. The direction of every ending, which is not dependent upon hydraulical processes, of which latter no other occurs to me at present, but the ending in resolution. But the efficiency in this case is secondary, and it is not the only one.

5th. The absorbents are influenced by the same cause: in consonance with a function which is peculiar to them, they remove the structure which is destroyed by the modification of life connected with the nerves, *in a ratio to its decomposition:* other effects may arise from the state of these vessels, but it is unnecessary to enumerate them.

6th. All the kinds of inflammation, and all the phenomena they display, are dependent upon chemico-hydraulic agencies; to which a direct-

ing principle of life, under a preternatural condition, is superadded. This is a fundamental proposition, and one from which a more systematic inquiry might properly originate.

7th. The chemical and hydraulical changes are subordinate; being regulated by a modified identity of life, as they are governed in health, whether with respect to the fluids, or the consolidated textures, by a natural identity of the same principle.*

8th. The mechanical appearances are principally governed by an hydraulic operation: the chemical processes are intermediate: the function of the nerves also respects affinities, which are vastly complicated.

9th. Pain results from a modified function of the nerves, and is not dependent upon dis-

* It is not the mere cessation of life which produces a slough. I believe that under some circumstances of the identity of this influence which is exerted by the nerves, a kind of inverse operation takes place upon the subjects of its former alliance, and that the slough so produced is essentially different from that putrefaction which succeeds to ordinary death; that it is, in short, a state of the sensible textures, which no other agent in the universe could produce, save that principle of life, and which the principle itself can produce only under one condition of it. After ordinary death, the integrity of the textures may be a long time preserved; a slough may be formed in a few hours, after the influence of a cause whose relation is with the principle of life.

tention: this is proved by the pain which accompanies the first access of inflammation in any part of the cellular structure of the penis (as well as by many other facts), where distention by blood is not so great as is at other times sustained without pain.

10th. The generation of heat. That an actual generation of heat on the surface may take place, independent of the contiguity of red blood, is proved by this frequent fact, namely, that persons, *pale* and emaciated, reduced perhaps, as by hemorrhage from the lungs, to the last stage of life, may be affected with a dry and burning skin. If the temperature of the surface in such cases depended upon the greater contiguity of red blood, in consequence of its being urged into the extreme vessels, this condition of the vessels, in a subject whose skin is almost transparent, should be indicated by redness: on the contrary, I have observed it *pale*, and yet very hot; which proves that some order of the structure of the skin, or rather the principle allied to it, is capable of assuming a condition under which there will be a generation of heat, independent of a derivation of caloric from blood, unusually contiguous to the surface.

These suggestions arise out of views, the full development of which must influence every

department and section of physiology: and they can find their proper support only in the elaborate exercises with which they are connected. In making however this sketch on the subject of inflammation, no pledge is given for the truth of the doctrines which it involves: it was professed to evince merely that the processes of disease are in consonance with the natural relations which were before designated; the example of inflammation was selected as comprehending phenomena, the perfect knowledge of which would leave but little to be regretted, either in the subject of physiology or of disease.

SECTION II.

DISEASES OF NERVES.

ARTICLE I.

Tic Douloureux.

OF the cases of tic douloureux which I have seen, I select the following as an example of the disease:

Richard Bragg, a brass nail-maker of Birmingham, forty-three years of age, about eighteen years since felt some slightly painful twitches on one side of the face. The pain of the disease in its incipient stage was sometimes exacerbated, and occasionally a remission or suspension of it took place, which lasted for a period of perhaps two or three months: a considerable time after the commencement of the affection, a more permanent suspension of it took place, which continued three years, at which period it again returned. The subsequent occurrences of the

disease were marked by an increased violence; and the intervals of remission became of shorter duration. At the time of my first seeing him (which was in the year 1808) he gave the above sketch of the history of his complaint, and added, that the remissions of the attacks had of late seldom continued more than a week or ten days, and that at those times his sufferings were but supportably mitigated; that the pain for the last two months had scarcely undergone even a temporary abatement: his days and nights were passed in the extremest misery; he described it to be an almost inconceivable torture, which resembled, to use his own expression, "the piercing of several sharp penknives," and during the whole of this time he had scarcely enjoyed an hour's connected sleep.

His body was very much reduced in bulk; there was a peculiar wildness and rapidity in the motions of the eyes; and the muscles of the face appeared singularly susceptible of the slightest impulses of the mind, the influence of which was denoted by spasmodic action of the corresponding muscles of the face. Whilst talking, he would suddenly put his hands to his face, making strong pressure upon the situation of the pain; and his agony would be expressed by writhing and contortion of the muscles of the whole body. The attack was sometimes pre-

ceded by pain all over him, beginning at his fingers' ends and proceeding to the face, where it became fixed and concentrated; the fits frequently commenced with rigors, which, when the attacks were over, were followed by profuse sweats. During the continuance of the paroxysm, his attention could scarcely be excited to external objects; the attack commonly lasted from one to three minutes, and was followed by an interval of about the same time between its cessation and recurrence: during this interval, the muscles of the face were sometimes successively affected with a tremulous motion. There was a trifling increase of temperature on the affected side of the face; and the integument, covering the prominence of the cheek bone, was commonly a little red.

He had resorted to all the remedies which had been recommended to him. He had taken opium in every dose; but it never produced a mitigation of his sufferings, unless taken in a quantity sufficient to induce either a state of profound stupor or of delirium. These large doses of opium diminished the acuteness of the pain, but smaller quantities rather augmented its violence. He obtained more relief from tobacco than from any other narcotic; and this he chewed so frequently, that he was often in a state approaching to syncope. He had also

taken arsenic in doses, and for a period sufficient for the purposes of the trial: on the first use of it, a remission of symptoms gave encouragement to proceed, but the attacks were very speedily renewed with rather an increased severity. Mercury had likewise been employed, but not to a sufficient extent to produce ptyalism. From all this medical treatment he derived no benefit.

Pressure on the facial nerve of the fifth pair produced a decided mitigation of the diffused pain in the face; but it sometimes occasioned an increase of it at the immediate point of application. A kind of instinct had instructed him of this fact; for, during a paroxysm, with his fist he would make the strongest pressure he could exert at the place of the infraorbitary foramen. The pain, though greatly affecting the side of the nose and mouth, was by no means limited to these parts: during the paroxysms, it was diffused, and darted in all directions over the affected side of the face. The principal course of it was from the left side of the upper lip, by the ala of the nose, to the orbit (where it was the most severe); from thence passing under the eye, it reached down to the lower jaw.

It was determined to divide the facial nerve of the fifth pair, which was accordingly done by

Mr. Freer, by one incision, made down to the bone, at the place where it emerges from the infraorbitary foramen.* The infraorbitary artery, which was also wounded, bled for a few minutes; but the bleeding ceased upon the application of a compress sticking plaster and a bandage.

The operation was attended with greater success than might have been expected: it was followed by a total cessation of all the symptoms of the disease. This is a remarkable instance of the exclusive disease of one nerve, in a case which had been so many years established. The man slept soundly on the night succeeding the operation; the sensibility of the face was a little diminished; and if the testimony of his feelings might be admitted, the temperature of it had likewise suffered a slight reduction. The wound united and healed in a few days, the man's health and comforts were restored, and he was enabled to follow his business without any interruption.

* I conceive there never will be any difficulty in accomplishing the division of this nerve. Dr. Haighton has attempted to define an average situation of the infraorbitary foramen; but the entire dependence upon such a mensuration appears to me objectionable. One of the best criteria of the place of the nerve will be found in the effects of pressure; with this assistance, and a good practical knowledge of an anatomy, it is almost impossible that the nerve should escape an incision, made down to the bone, and embracing an extent of $\frac{3}{4}$ of an inch.

I saw this patient about eight months after the division of the nerve. The place of the incision, though quite healed, was covered by a piece of black sticking plaster, which he had habitually worn, in order to prevent an unpleasant sense of coldness, which regularly occurred upon his leaving it off. He informed me that, for three months after the division of the nerve, he had been entirely free from pain; but that at about this period he had some slight return of his former sensations, and these had been increasing both in frequency and acuteness up to the time of my seeing him. He anticipated the necessity of another operation, but delayed resorting to it because he was still able to manage his concerns.

Dr. Haighton remarks, that it is a particular upon which the success of the operation must very materially depend, whether the disease precludes the division of the nerve above the place of the origin of the pain, and that to which the affection subsequently extends; unless this can be accomplished, there is little probability of operating with success. When the commencement of the attack is not confined to one point, and extends in many directions, there is no reason to expect any great advantage from the operation.* This observation appears

* Med. Records and Researches.

very reasonable; but it is shewn to have some exceptions by the facts of the preceding case.

With respect to the ultimate pathology of tic douloureux, we possess no satisfactory information. That it is a disease occupying specifically the nervous structure, may be inferred from the well known dependence of sensation upon the nerves and their centres, and is additionally proved by the events of the division of the affected trunks, which was suggested as a means of cure, by a recollection of this dependence.

It has never been found that the nerve has been in a state of inflammation, nor have we any reason to deduce such a conclusion *à priori*. When a nerve is inflamed, the pain is continued; it takes place in common wounds, and is of a smarting kind; it may become more intense, but it would still possess the characteristic symptom of being continued. Acute pain also takes place in muscular structure; it may endure for three or four seconds, it may then cease, and never again be felt. There are few people, who have not in their own persons experienced a spasm of this description. That this criteron furnishes a diagnosis as far as it has been assumed, is supported not only by collateral proofs but by the experience of injuries of nerves, which

have been followed by inflammation. The pain which accompanies inflammation in the mixed structures is cotemporary with the other processes of inflammation; it is not suspended and renewed, though liable to be mitigated, or to be exacerbated.

We appear justified therefore in the opinion, that the rapid alternations of acute pain and perfect ease are incompatible with the presence of inflammation in a nerve. But while we assume this as a distinction sufficiently clear, it is not designed to affirm the general proposition, that the question which might arise upon the presence of inflammation is always to be determined by the duration of the pain; or, in other words, that all pain which is continued is the result of inflammation. We are assured from dissections that many acute pains are felt in various organs during life, in which the examination after death discovers no traces of inflammation. We should perhaps be gratified to find in tic douloureux that the nerves had undergone inflammation, because this is a familiar acquaintance; we flatter ourselves that we understand it, and its detection prevents us the trouble of further inquiry. It is with the same feeling that we turn our regards to the changes of structure, which also afford us a satisfactory explanation, though the symptoms are rarely con-

nected, by observers, with the organic change: as this however is a condition which will be naturally looked for in the disease we are considering, it is right that the grounds of the supposition should be examined.

That a change of the structure of a nerve may modify the function which belongs to it, there can be no doubt: the fact is exemplified in the tumours of nerves, in the effects of the ligature, and in their lesions: it is true, the visible organs of the system are subordinate; but they are instruments, and the relations of structure are indispensable for their function; the changes of the textures of nerves, though secondary, are capable, by affection, of influencing their invisible alliances: but that tic douloureux is a disease involving a change of structure, either of the nerve itself, or of its centre, cannot be assumed, for these reasons: 1st, Because such a condition of the structure has never been remarked in the disease; 2d, Because acute pain, alternating with remissions, or suspensions, elsewhere frequently occurs, where no change of structure is to be perceived. On these accounts, the condition we are considering, and the symptoms of tic douloureux, cannot be regarded as a necessary connection. We perceive here an absence of direct information; but the evidence of frequent analogy (to which no

exception appears) tends to negative the conjecture.

On the other hand, there is reason to suppose that minute changes of structure occur in nerves, by which their function is liable to be modified: indeed so frequent is the connection, that it may be affirmed we know of no instances, or of none but those which are trivial and doubtful, of the palpable de-organisation of a nerve, which is not accompanied by the symptoms of a disordered function; and, correspondent with the cause, these symptoms will be likewise permanent. To the testimony of Meyer I can add my own, that a change of structure occurs at the place of re-union of a nerve; and that an irregularity of function may be concomitant with such organic change, appears from the following case. The circumstance of being myself the subject of the accident, will not detract from the accuracy of the sensations as they are described.

About eight months since, I cut the outer side of the fore finger of the left hand just behind the first joint; the incision was oblique, and extended down to the periosteum. The wound was dressed with sticking plaster, it united, and got well in less than a fortnight. A trifling inflammation remained around the edges of the

cicatrix after the final removal of the dressings, and a slight redness at the former place of the wound has been perceptible ever since. I discovered that the outer side of that part of the finger which is anterior to the wound, had lost its sensibility, and the loss of this faculty extended to the extremity of the finger.

About a month after the wound was healed, I felt, on pressing that part of the finger which had been hitherto insensible, a sense of pricking at many points, which however was only of momentary duration. The sensibility of the finger has since that time been gradually increasing: and now a degree of inflammation, which was before limited to the cicatrix, has extended itself over so much of the finger as was deprived of its sensibility, and is displayed by redness of the skin, sometimes accompanied with a slight elevation of the cuticle at two or three points. If this surface is casually touched, a sharp and diffused pricking sensation is produced; and the sensibility of the part is such, that the sympathetic sensation which we call setting the teeth on edge is induced many times a day, by the finger being made to pass lightly and accidently over my clothes, or any other rough surface. The motions of the finger are perfect; and the pain of it is excited only by contact with external bodies.

In this instance, the division of that branch of the radial nerve which passes on the outer side, to the extremity, of the first finger, produced the insensibility immediately consequent upon it. This nerve afterwards united; and the re-establishment of its continuity has been productive of the irregularity of function which I have just described.*

It is hence indicated and proved, by more striking examples which have been before adverted to, that the functions of nerves may be disordered by the presence of organic change: but as there is no evidence which shows that tic douloureux cannot take place without the accompaniment of a disordered structure, there being also no example of its having been connected with derangement of structure; and as the analogies are, to say the least of it, as favourable on one side of the question as on the other; we appear justified in concluding that tic douloureux may take place without being the consequence of perceptible organic change; that a disease of function, in some respects similar might also be connected with organic change;

* More than twelve months have now elapsed, and the sensibility of the finger is not yet natural. It is however less susceptible than formerly, having become habituated to the causes of irritation.

and where such change is palpable, we cannot but remark this characteristic circumstance, namely, that the effects of such a condition of the nerve (like the condition itself) are permanent. Assuming therefore that we possess the sanction of data thus far, it remains to be examined, as the pathology of tic douloureux does not find its development in the changes of structure, in what other way the symptoms of the disease can take place?

To enter fully into this discussion, would involve many speculative principles of physiology, which have never been glanced at in the scrutinies of processes. As a theme of disquisition so copious cannot here be engaged in, a few remarks only will be offered on the topics, and order, of the inquiry.

The disease we are considering consists in an affection of the faculty of sensation. The origin of this affection is spontaneous, and may occur independent of any perceptible alteration of the material organs, with which it is allied: by what mode then does the affection of a faculty, which belongs to the nerves, *commence?*

We perceive the possibility of two origins, which correspond with a double function; one belongs to the nerve, and the other to its centre.

But that the disease we are considering is one which arises in the nerve, appears to be confirmed by this fact, namely, that the division of a trunk, will suspend the symptoms of the disease, although that trunk may give off branches, also susceptible of the disease, which are more contiguous to the centre of the system; or, in other words, if the communication of the affected branch (a point of origin) with the brain, be intercepted, the disease will not affect other branches, unless they are also become the seat of it.

Dr. Darwin has remarked, that the seat of the disease is also the seat of the affection: but this observation, as an unlimited one, is incorrect; for in the case of Bragg a *complete* cessation of the disease succeeded to a division of the second branch of the fifth pair, where it escapes from the infraorbitary foramen, although the pain was the most acute both behind and below this point, namely, within the orbit and extending down to the lower jaw. This fact furnishes a good proof of the locality of the disease, and is an example of the assumption that the origin of tic douloureux *is in the nerve, and not in the brain.*

But how, it is next to be inquired, can a disorder of a nerve arise without an assignable cause? There is not, in the whole range of

pathology, a more difficult or a more important question to be fully understood, and to be connected in the different examples, than this.

The affection of an individual nerve is either primitive or secondary. When primitive, the mode of the origin of such affection must be either by a change of the *relations* of the modified faculty, or by a change of the *faculty itself.* The order of investigation suggests that we should now recur to our physiology, and consider the quality and number of these relations.

We are informed by our sensible testimonies of only three conditions, conspicuous in the substance of a nerve, which are necessary to concur with the faculty, which last is not discerned: these respect the fulness of vessels, the constitution of their fluids, and the arrangement of solid particles. In which of these shall we look for the origin of disease? or shall we expect to find it in the *faculty*, which, as may be demonstrated, is not itself simple and elementary? In truth, we have no reason for preference: and we may ask of each, why should the fulness of vessels assume a change *in this place?* or why the constitution of fluids? or why the arrangement of solid particles? or why the principle of sensibility?

Pursuing the same clew, we shall conceive that there might be subordinate or preparatory changes, furnishing predispositions not perceived, which end, by a very natural catenation, in diseases which are perceived. But this resource does not remove the difficulty, because it is still necessary to suppose a first change; and, in seeking for it, it is superfluous to inquire beyond the earliest period of the subject we contemplate. In some other exercises, which need not here be expressed, I have thought it necessary to connect the origin of spontaneous disease with an idiosyncrasy, which was determined before the first act of the development of the ovum. We will not however stipulate so precisely for the period, but we will say previous to the commencement of the functions of an animal.

In examining the other alternative, namely that origin of diseases which has been called secondary, as by communication from another seat; our conception of one part of the process will be easy, while that of the other will involve the difficulties just mentioned, and which it has not been attempted to explain.

If a natural relation subsist between remote parts, as it evidently does, it is in perfect harmony with the fact, that a deviation from an original condition which begins in one organ, or

in one part, may variously affect those others with which it is related; as the prick of a needle in an extremity may affect the whole muscular system with a sudden action, alter the course of thought itself, and excite speech or variously modify the movements of articulation. In this case, the origin of the change of condition (which may be from one of perfect ease) is clear; it is traced to an external cause: but this is not the process of spontaneous disease; and, generally speaking, if diseases be traced with great minuteness and curiosity from one cause and from one seat to another, the examination will still end in a difficulty respecting some or other of the questions involved in the origin, the disposition to which will be referred to a very remote period of the constitution. This is a difficulty which is naturally to be expected, where the circle of connections is almost (or, for aught I know, *quite*) without end.

From this view of the matter, the possibility of strictly spontaneous disease, not traceable directly or indirectly to external causes, may be almost doubted; but the doubt will not remain, when we recollect the effects of idiosyncrasy, which show themselves at certain periods, when all the other causes to the operation of which the subject might be exposed are the same to day as

yesterday, and have been familiar for years. In physiology many examples of the fact may be cited; I shall mention only one, namely, the attainment of puberty.

These remarks would be rendered more striking, and perhaps more comprehensible, by a more copious illustration; but as the examples are numerous, and for the most part may be found almost without seeking for them, it is unnecessary to extend a disquisition which may perhaps have been dispensed with without any loss.

The indications of the cure of tic douloureux are of two kinds: the first respects the subversion of the cause of the disease wherever it might be found, compatibly with the preservation of the function of the nerves. As this cause is not known, and as the mode of its first influence is also latent, we are directed by no principles in the selection of remedies, save those which we deduce from distant analogies. The second indication respects the prevention of the disease, by producing the extinction of the faculties without which it cannot take place, or, having taken place, cannot be maintained. We shall offer a few remarks on these respectively.

A direct principle for the cure of tic douloureux by medicines is wanting; the treatment

has therefore been directed by distant analogies. Arsenic has been tried to a great extent, upon no better presumption than that it is a powerful agent. Mercury has also been employed in considerable quantities; not merely because it cures syphilis, but because it is likewise said to cure some other diseases. Many medicinal agents, the acids, ammonia, camphor, and the anti-spasmodics as they are termed, have been applied to the cure of tic douloureux, upon no better foundation. It is unnecessary to add that these have failed.

The use of the narcotics appeared to be indicated by an analogy of a more connected kind; accordingly opium, tobacco, hyosciamus, and cicuta have been given for the cure of tic douloureux; by the former it is not assumed that any lasting benefit has been conferred. Dr. Jackson, of Boston, reports that he has exhibited as much as 300 grains of the extr. conii in six hours; but the constitution of his patient had become accustomed to the action of the remedy. He thinks however that the increase of it should not be too gradual, as it is necessary to produce a sensible effect: this effect is manifested in 15 or 20 minutes after the medicine is swallowed; and the dose which produced it might be repeated with safety after the interval of a few hours. Dr. Jackson commenced his treatment in one case, by giving a pill of five grains to be

repeated night and morning, with the addition of one pill to each dose, until sensible effects were produced. I cannot however concur in the exhibition of so large a dose of the cicuta from my own experience of it; six or seven grains have appeared to me a full dose; repeated every six hours, I have found it to produce sickness and vertigo, which did not supervene in less than an hour, and continued so long as to prevent the repetition of the medicine at the proper interval. The difference might perhaps be in the strength of the extract, which I have reason to think is very irregular. Upon the whole, this remedy appears worthy of further trial; and if I were called upon to suggest another upon no better principle, I should recommend the continued use of purgatives. In a recent publication I have seen a report of the employment of bella donna in a case of tic douloureux: this treatment was instituted by Mr. Kerrison, who gave the extract in doses of a quarter, increased to one third, of a grain, apparently with good success.

Experience has afforded us no great reason to be sanguine in the result of any known medicinal agent: we may therefore proceed to consider the second indication, namely, the cure of tic douloureux, by the prevention of the faculties which are necessary to the existence of the disease.

As the faculty of sensation is specifically interested, and as the possession of this faculty by the nerves is dependent upon their integrity, and their communication with the brain and medulla spinalis, it must have appeared an obvious means of cure to destroy their integrity, and to intercept this communication. The operation has accordingly been frequently performed by dividing the trunks of affected nerves; and the history of the practice furnishes many memorable examples. But it has been found that after the lapse of a period, which is not the same in all instances, the disease has returned. The recurrence of the disease was reasonably assigned to the re-union of the divided nerve, and to the consequent renewal of intercourse with the brain. It then became a desideratum, of as much importance as is to be attached to the cure of one of the most painful maladies with which human nature is afflicted, to prevent the re-union of the nerve.

The retraction of nerves, before spoken of, does not appear to be a fact which was generally known; and by those who reasoned without the aid of this information, it might have been presumed that the truncated extremities of the nerve would remain after the division in a state of close approximation, liable to re-produce the continuity of the nerve by a slight exertion of

that faculty of regeneration which is common to the other textures, and exercised conspicuously by the filaments of nerves in the filling up of deep ulcers, and in the rapid growth of a fungus capable of sensation.

But although the nerve would unite when the extremities were nearly contiguous, it did not follow that a greater extent of nerve would be re-produced; accordingly, a portion of the affected nerve has been removed, but not with the success which was desired. A case of this kind is related by Mr. Abernethy,* the particulars of which, as they are also illustrative of the subject in some other respects, I shall beg leave to abridge. The subject of the disease was a lady: the pain was originally seated under and on the inner edge of the nail of the ring finger of the left hand. This affection came on spontaneously, and the fits of its recurrence happened without the operation of any external causes: the pain however was exacerbated by any slight and casual injury. Seven years after the comencement of the disease, the skin covering the place of the affection was destroyed by caustic. This treatment produced excessive pain, and the misery of the disease was afterwards rather augmented than diminished. The pain

* Surgical Works, vol. ii. p. 203.

extended up the arm, and the muscles of the neck became affected with spasm.

Eleven years after the first appearance of the disease, Mr. Abernethy laid bare the nerve of the finger by an incision of about three quarters of an inch in length; and divided the nerve, by passing a bistoury under it, opposite to the second joint of the finger: having cut through the nerve, he afterwards removed a portion of it, half an inch in length.

The wound became painful, and it inflamed at the place of the upper cut extremity of the nerve; but the inflammation disappeared in about three weeks, and the sensibility of the finger was so far destroyed, as not (in the former seat of the disease) to be susceptible of pain from pinching &c. the skin, though an accidental movement of the finger would produce it.

An imperfect sensibility of the extremity of the finger was restored at about the end of three months from the division of the nerve; and the faculty continued to improve, so as to enable the patient to discriminate with that finger the tangible properties of things. Slight pressure on the former seat of the disease, at about the end of nine months, produced sensations resembling those which had been formerly ex-

perienced, and the re-establishment of the disease was expected to take place. It has been before remarked in discussing the question of metastasis, that the growth of the nerve in this case, supposing its re-union to have been accomplished, corresponded with that which was observed in the experiments of Meyer.

As a means of temporary relief, for sufferings which amount to the greatest agony, the division of the nerve in tic douloureux should be recommended. This recommendation must arise out of a comparison between the pain of the operation and that of the disease: in a severe case of tic douloureux, the alleviation enjoyed after a division of the nerve in the next 24 hours is a sufficient compensation for a momentary increase of pain. But the mitigation is in no instance so evanescent as this, where a total division of all the affected nerves has been accomplished. The re-union of a nerve will not take place until a distant period, and even then the disease does not immediately return. Many months have elapsed between the division of the nerve and the recurrence of the disease: as the union of a nerve, according to the facts which we possess, may take place in six weeks, or, as I have reason to think, in a shorter time, we must conclude that some change is induced in the nerve at the place of union, which prevents the

phenomena of the disease, and requires the intervention of gradual processes for the restoration of its natural condition. But we are not certain of this effect, and it is not therefore to be at all times calculated on; for it has been known that the symptoms arising from a diseased state of a nerve have been aggravated after its division. To cite only one example, this increase of disorder evidently supervened in an interesting case of diseased median nerve, related by Sir E. Home,* upon which we shall hereafter have occasion to remark.

But as we reckon only upon probable results, which must be deduced from the majority of facts, the following appears to me a principle of surgery which is fully justified, namely, that if tic douloureux can neither be cured, nor from time to time mitigated, by medicines; and if the pain be of the severest kind; and if no other means can be instituted which will *permanently* intercept the central communication of the nerve; it then becomes a duty to recommend and practise that operation, which will probably suspend the sufferings of the patient for a considerable period, and which might be repeated as often as the same necessity shall arise. The end of the neglect of such a recommendation

* Philosoph. Transact. Royal Soc. 1801 (Crooman Lecture).

(an example of which has fallen under my own cognizance) is, that the patient exists miserably for many years; and at length death, long prayed for, arrives, and puts an end to the torture by which the event was progressively occasioned. Will it be said it is much better to die, than to have life preserved, by a formidable operation, the repetitions of which cannot be anticipated? To this I reply, that life is not worth having upon such conditions, if the period of suffering would otherwise be short: but this is not the case. Further, it is no part of the business of a surgeon to estimate pain: the end of his profession is to prolong life; it is for the patient to acquiesce in, or dissent from, the terms.

While we rejoice in the possession of so good a palliative, it is a design worthy of some consideration, to prevent *permanently*, by a single operation, the faculties of the nerve, which are essential to the disease, and which depend upon a free communication with the centre. I therefore submit the following experiments, which were made with this view; and which, though not so successful as may be wished in regard to the end of their institution, may show so much of the tendencies and phenomena which take place in nervous structure under preternatural conditions, as will eventually lead to the attainment we desire. All the steps of progress, from

the commencement of research, to its perfection, are equally important. "Namque alid ex alio clarescet" &c.

From considering the structure of the nerves, from considering their diseases, and from recollecting the vessels which Reil has described, which ramify between the bundles of their cylinders; I was induced to think that the production of nervous matter from a divided extremity, may be prevented by substituting the ligature for the knife. It appeared reasonable to expect that a circumference of the nerve would be destroyed, which would be comprehended in just that circle upon which the action of the ligature had been specifically exerted. That this destruction of the nerve would allow the separation of the ligature, and that the portion of the nerve immediately on either side of it would have undergone a contact and pressure, which would induce the adhesive inflammation, thus obliterating the tubuli of the nerve in nearly the same manner as the ligature accomplishes the obliteration of an artery.

In order to confirm or refute this supposition by experiment, I exposed the axillary plexus of a rabbit, and included one of the largest nerves within a ligature composed of three common-sized threads: this ligature was drawn rather

more tightly than is common in securing arteries. I had previously ascertained the degree of pressure which a nerve will in this way sustain, by a ligature on the optic nerve of a sheep; and I found that the theca of a nerve is capable of bearing, without rupture, the greatest degree of pressure which it is possible to make. The nerve which was tied singly was situated on the inner side of the axillary artery. There were three considerable branches of the plexus, lying close together, and situated above this vessel: these were included within one ligature composed of two threads, with a view of ascertaining whether the results of the ligatures would be the same in both instances, and also to attain a double evidence of their effects. One small nerve, which was situated on the outer side of the artery, was tied with a single thread.

The atmospherical temperature was 65: on examination five minutes afterwards, I found that the heat of the foot of the opposite leg raised the mercury in a thermometer up to 80, while it sunk to 72 on being applied to the foot of the leg, the nerves of which had been tied. No conclusion is perhaps to be deduced from this reduction of temperature, as a small branch of the axillary artery was divided, around which it was necessary to pass a thread; and before this branch was secured, the leg had been de-

prived of perhaps two or three drams of blood: whether the reduction of temperature was in any degree referable to this circumstance, I will not here attempt to determine: we know, from practice, that a reduction of the temperature of a limb follows the obliteration of the principal artery.*

The tying of the ligatures produced considerable pain, which appeared to be only momentary; the leg was immediately rendered motionless and insensible. There were two smaller nerves of the plexus, the natural communications of which were preserved. These were mere filaments, and did not take their course down the leg, as if for an extensive ramification; they appeared to correspond with the circumflex and supra-scapulary nerves of the human subject. The animal did not experience any great derangement elsewhere; he soon began to eat, and his vivacity was not at all diminished.

The pulsations of the brachial artery were distinctly ascertained after the application of the ligatures; but the leg never regained its former temperature. About the 5th day from the

* As the examination of temperatures sometimes formed a collateral part of experiments, the remarks on this subject are mentioned incidentally.

experiment, the skin covering the foot sloughed off, and the same process was gradually extended to within two inches of the situation of the ligatures.

The destruction of the integument was complete, and the bones of the leg were exposed nearly up to the shoulder by the 14th day after tying the nerves. During this period I occasionally attempted to bring away the ligatures, but found them all firmly attached, except the one which secured the branch of the artery, which separated in less than a week. The animal's health was unimpaired; and he appeared as unconscious of the presence of the leg, as if it had been totally removed.

The sphacelation of the integument was a circumstance worthy of remark; it is one upon which I have elsewhere grounded an argument: a similar connexion is occasionally indicated by the cases of surgery.

On the 14th day the rabbit was killed. The ligatures were still fixed. The two first had destroyed the nerves *below* the places of their application, and not a vestige of them could be perceived: these ligatures were attached to the superior extremities of the nerves. The third ligature had produced the destruction of the

superior portion of its nerve, and was still attached to the inferior portion, which could be traced as far as the line which marked the termination of the mortification.

A simple division of the nerves by the knife would have had only the effect of intercepting their communication with the medulla spinalis. The ligatures accomplished more than a simple division; they produced actions which destroyed the inferior distribution of the principal nerves: the indications of this fact have been fully discussed.

Oct. 12th, I included one of the largest nerves of the axillary plexus in a ligature composed of two threads. The pain was acute, but of only momentary duration; and a slight lameness ensued. The health of the animal continued unimpaired: the ligature separated on the 18th, and the wound was completely healed in less than three weeks.

Dec. 1st (at which time no perceptible lameness remained), the rabbit was killed. On examination, I found that an extent of half an inch of the nerve had been destroyed by the ligature. The extremities of the nerve were a little conical, but no filament was extended from either of them, nor was any indication observable of a

tendency to reproduction. These extremities were examined with a magnifier, and the continuity of the membrane which confines the medulla appeared perfect. There was no perceptible margin of its termination; and pressure in the direction of either extremity of the nerve did not produce an escape of its medulla. On drawing the nerve with a strong pressure through my fingers, the extremity first underwent a dilatation, which was followed by a sudden effusion of its medulla, as if in consequence of the rupture of a membranous pellicle. The colour of the nerve had undergone no very sensible change, except near the point of its termination; which probably happened from the destruction of all that portion of it which had undergone a stronger degree of inflammation.

The period which is required for the final effect of the ligature is shown to be one of considerable irregularity. On a former occasion, the ligatures were found firmly attached at the expiration of more than a fortnight: the other consequences of their application were nearly analogous to those in the instance just mentioned. I have uniformly found (when sufficient time has been suffered to elapse) that a sloughing, or destruction of a portion of the nerve, supervenes upon the use of the ligature. I offer no attempt to explain the varieties which are manifested in

different experiments; it is probable that they are in part referable to casual circumstances, which, from the obscurity of phenomena, are liable to be overlooked.

Although the separation of a ligature from a nerve be attended with circumstances very different from those of its separation from an artery, the extremities of the nerve present something the appearance of an obliteration: they are conical; and I have been able to discover no marginal termination of the theca near the extremest point. The destruction of the nerve is not confined to the immediate circle of the ligature: the distance by which the extremities are afterwards separated is not uniform; and the lateral portions of the nerve, which are immediately contiguous to the ligature, do not assume the adhesive processes.

But it is indicated by the result of the last experiment, that the inflammation of the nerve may extend beyond the place of its destruction;* and that a continuity of the theca may be established either by a production over the medullary surface, or by a collapse, and subsequent adhesion, of this membrane, tending equally to prevent the re-establishment of the natural con-

* If the discolouration of the extremity may be assumed a test.

tinuity of the nerve. To speak decidedly on this point will require the illustration of additional experiments, the results of which would be the most satisfactory, if made on a larger animal. In the investigation of Meyer, we find that either re-union of a divided nerve, or the evidence of a tendency to reproduction, was displayed in a shorter period than was allowed for a similar consequence, after the application of the ligature.

The function of a nerve depends upon its *perfect* continuity. I would not be understood, that this dependence is in the relation of cause and effect; that the continuity produces the power of the function (which has in effect been asserted); but that the perfection of the nerve is required as the instrument of the function, and as a medium of intercourse with the external world. If the theca of a nerve be wounded, its function is modified or altogether suspended; if the medulla be displaced, its phenomena of sense and motion can no longer be produced.* This latter then will constitute the state of the nerve we are solicitous for; the only point to be thought on is, to render this interruption in the continuity of the medulla permanent. It is

* This fact will be mentioned more fully under the article of Inflammation of Nerves.

obvious that the same, or perhaps a more effectual interruption of the continuity, would be occasioned by a total division of the nerve, provided such division could be made permanent, which is not the case in ordinary instances. Two modes of accomplishing this design suggested themselves to me, of the results of which I shall give a faithful account.

It appeared feasible to intercept the divided portions of a nerve by mechanical interposition. I therefore divided one of the brachial nerves of a rabbit, and introduced the lower portion of it into a leaden tube of correspondent size, closed at one end, and about half an inch in length. I thought to have fixed this tube by the pressure of a compress and bandage; and at the end of the experiment, I had every reason to believe that this design was accomplished. I thought it probable, that if the situation of it could be preserved for a short time, adventitious substance would accumulate around it, and thus preserve its place after the wound was healed. In this effect I was disappointed: actions of the muscles, which could not be prevented, displaced the foreign body, and it finally made its escape at the wound.

I believe that this is still a practicable method; it would however be applicable only

to nerves, which are sometimes, though rarely, affected with disease, as those of the extremities. A fine thread may be passed through the theca of the nerve, and brought through a tube, which may then be closed at one extremity upon the thread: this contrivance would perhaps fix both the tube and nerve, until time had elapsed for the thickening of adjoining structures, which I have reason to think would take place. As the subject of this process would be the lower portion of the nerve, there is nothing to fear from irritation, &c.: but the question afterwards remains to be decided, whether the lower portion of nerve would live under these circumstances? That the external wound would readily heal, notwithstanding the presence of such foreign substance, is indicated by the fact, that people frequently carry a charge of shot to their graves, which had been lodged in the skin at an early period of their lives.

At the time that this last experiment was made, another of a different kind was also instituted with the same view. I had before found that a ligature, which was suffered to continue for any length of time upon a nerve, produced a destruction of it to some extent. It appeared probable, that the *temporary* action of it may not only displace a circle of the medulla, but excite at the same time the adhesive inflammation in the neurilema; thus precluding the

restoration of the continuity of the medulla, by the processes of growth. In order to ascertain this point,

June 24th, the four largest nerves of the axillary plexus of the right leg of a rabbit were tied with a broad ligature, about half an inch from the thorax, and the artery was carefully excluded. I suffered the ligature to remain on seven minutes, and then cut it away; which was accomplished without wounding the nerves. The nerves were then separated by passing a blunt needle between them, in order to prevent the effects of the compact they had undergone by the ligature. The medulla of each nerve was displaced, and a circular indentation of the ligature was preserved. The leg afterwards was found to be insensible, and totally incapable of voluntary motion.

The leg diminished a little in size, and remained completely paralysed at the end of six weeks. I was sanguine in the result of this expedient: but here again I was disappointed of the success, in the belief of which I had with so much satisfaction indulged. On the 18th of August the powers of the limb were improved: and by the 23d the animal was able to use this leg, apparently with as much facility as either of the others.

At this time he was killed; and, on examination, I found that the natural condition of the nerves was perfectly restored. They were a little discoloured at the situation of the injury (probably owing to the inflammation which had taken place); but their continuity was perfect. The ligature, therefore, had produced no adhesion of the neurilema, and the incapacity of the limb continued only so long as was required for a reproduction (or perhaps an extension, as there was no *loss* of substance) of the medulla: this period nearly corresponds with that which is required for the re-union of a divided nerve. If this process of growth, and every question it involves, were minutely understood, physiology would have but few secrets. I do not believe that *any interstitial substance*, merely restoring the continuity of the cylinder, would have transmitted the influence of the brain to inferior parts, and have perfected the properties of the nerve; for the alliance of these properties is with the substance which they affect, and which they govern, and produce. This fact is most extensively connected, but the disquisition is speculative.

Although, in this instance, the end of the experiment was not attained, I was disposed to believe it practicable, by a slight modification of the same process. It appeared probable that the ligature had not been suffered to remain a

sufficient time for the establishment of the adhesive inflammation.

August 26th, I tied the left sciatic nerve of a rabbit with a broad ligature composed of four threads: it was suffered to continue on the nerve forty-eight hours, and was then cut away. The part of the nerve embraced by the ligature did not appear ligamentous, as it had in other instances been observed; but looked as if the upper could be freely moved upon the inferior part of the membrane, and as if the processes of destruction were already commenced, which was further indicated by a point of medulla which escaped from the margin of the circle of the ligature.

Oct. 28th, the nerve was examined, and the natural condition of it was found to be *perfectly restored*, except that it was enlarged in a trifling degree at the place of the application of the ligature. These results prove that the prevailing propensity of the neurilema is to produce medulla; that this effect will take place under any inflammation short of that which accomplishes the destruction of the membrane; and that the effusion of coagulable lymph, by which adhesion might take place in the structure of nerves, is to be considered an effect of accidental predisposition, or of disease.

Does there then remain, it will be inquired, a probable resource for intercepting permanently the central communication of nerves, without the knowledge of which, the practice of surgery, in regard to this system, must remain defective? To this question I reply that I am acquainted with none; unless it should be found in some modification of the preceding attempts, or unless it should be indicated by this proposal, namely, to remove a portion (as half an inch) of the nerve, and unite the contiguous surfaces, one of which was above, and the other below, the course of the nerve, by an exclusive pressure, operating specifically upon the parts between the extremities of the nerve. As the union of the surfaces (if they could be made to unite) would take place sooner than the reproduction of the nerve; the adjoining structures may thus be made a septum, by which the re-establishment of the continuity of the nerve would be prevented, as long as this interposition might remain.

ARTICLE II.

Tumors of Nerves.

IN no instance are we more forcibly reminded of the connection between the function and the structure of nerves, than by those palpable changes of the latter, which are occasionally furnished in the examples of adventitious growths. It is shown by some of these instances that the effusion of lymph from the vessels of nerves, is a process which may take place to a considerable amount; and this occurrence is one which we should naturally expect in an order of animal composition, the phenomena of which are in many other respects analogous to those of the mixed structures. This capacity for the effusion of coagulating lymph, furnishes the grounds of an attempt at preventing the influence which is obtained by cerebral communication, in the way which has been before designed.

The tumor of a nerve is a re-agent: its cause is to be looked for in that state of the vessels, which is dependant upon a primitive modification of properties which are allied with the nervous organs.

Cheselden, in his Anatomy, has given a short account of a tumor of the cubital nerve, which was found to consist of a transparent gelatinous fluid, over which the fibres of the nerve were expanded. As such diseases of the nerves are not frequent, I subjoin the following, which fell under my own observation.

John Lucas, 45 years of age, was admitted into the Birmingham hospital on account of a tumor, the situation of which corresponded with the centre of the os brachii. This tumor had been of progressive growth: it was perceived about five years previous to his admission into the hospital, when not larger than a small horse bean, and had increased to the size of a pigeon's egg.

The first appearance of it had not been preceded by pain, or spasm, affecting the muscles of the arm; nor could he attribute it to a blow, or other accident. From the time of its formation an increasing numbness had been experienced in the arm and hand, and the faculties of sense

and motion had suffered a very considerable impairment.

Upon examining the tumor externally, its size could be very clearly ascertained: pressure occasioned great pain, which extended both above and below the disease. No suspicion of the real nature of the case arose in the minds of the surgeons; the tumor was considered a deep-seated steatoma, and the phenomena of it were referred to its proximity to the course of a nerve. With this view of the matter, the only difficulty which was anticipated in the operation arose from the risk of wounding the brachial artery.

The tumor was, after a little dissection, exposed, by an incision of about four inches in length: its connections above and below were ascertained, and it was found to be a disease which existed in the substance of the radial (or median) nerve. It was necessary to separate the adhesion, which the whole surface of the tumor had acquired, to the surrounding parts, with the knife; and this was cautiously and safely accomplished. A bistoury was then passed under the superior connection of the tumor, with which the nerve was divided about ¾ of an inch above the place of its enlargement, and a similar section was repeated at about the same distance

below it. The portion of the nerve which was thus cut away was about three inches in length.*

The circumference of the tumor was the greatest in the centre; and it gradually diminished both above and below this point, until it was as it were lost in the natural size and structure of the nerve. On cutting into it, an expansion of the substance of the nerve appeared to form an imperfect cyst (for the distinction was not every where complete) in which it was contained, and the fasciculi of the filaments of the nerve were continued over it. The substance of the tumor appeared like a composition of medullary and fatty matter; and its firmness was increased by the intermixture of a cellular tissue.

The division of the nerve was of course productive of considerable pain. The wound of the integument was closed by sticking plaster and a bandage. On the second day, the arm was swelled and vesicated; the bandage was removed, and an evaporating lotion applied constantly, under which the inflammation abated. The union of the wound by the first

* This operation was undertaken, and skilfully performed, by Mr. Bowyer Vaux.

intention did not take place; but the secondary healing processes went on favourably.

The numbness and incapacity of the arm were much increased by the removal of this portion of the nerve; no constitutional affection however ensued, and the incision healed in less than eight weeks, at which time the man left the hospital. During the last three weeks of his abode in the hospital, the condition of the arm had undergone a visible improvement; its motions were in great measure restored, and the sensibility of it was likewise considerably augmented.

In this case, the production of such an extent of nerve could not have been accomplished in so short a time, and yet an improvement of the faculties which we know to be dependent on the nerves had certainly taken place. We have before examined the question of the manner in which this increase of power may be acquired, in the article on the Relations between Nerves.*

A singular case of injury, producing tumor of

* In order to complete the history of this case, I have lately written to Birmingham; and have been favoured by Mr. Vaux with the additional information, that he saw his patient six months after the removal of the portion of nerve, and that the powers of the arm were then so far restored, that the man sustained little or no inconvenience in the use of it.

the radial nerve, is published by Mr. Denmark, surgeon to Haslar Hospital.* A man was wounded by a musket ball, which grazed the bone of the arm an inch and a half above the inner condyle. The wound healed; great restlessness and pain supervened, which opiates were inadequate to allay. The fore arm and wrist were bent, but the latter could be extended with some increase of pain. A small highly sensible tumor was perceptible at the place of the wound. The pain was a burning sensation; it commenced at the extremities of the thumb and all the fingers except the little one, and extended upwards to the place of the wound, thus identifying the seat of the disease.

It was proposed by Mr. Denmark to cut down to the nerve, and remove a portion of it "above the wound." This was objected to by the other surgeons of the consultation, and the arm was amputated. The man was discharged cured in three weeks.

On examination, the nerve was found closely attached to the surrounding substances: it was thickened, both above and below the place of the wound, to about twice its natural size, and ap-

* Medical and Chirurgical Transactions, vol. iv.

peared contracted in its length. A piece of the ball, which had been driven off by its collision with the os brachii, was discovered in the posterior part of the nerve. In conclusion, Mr. Denmark asks, " Would the division of the nerve, and cutting a piece out of it, have been attended with success?" We cannot absolutely pronounce what would have been the event of such a project, but we may certainly anticipate some alternatives.

If a portion of the nerve had been removed " above the wound," it is most probable the reunion of the nerve would have taken place; and the continued presence of the foreign body, or its effect, namely, the tumor, would have secured the return of the disease, as the function of the nerve dependent upon its superior connections would likewise have been resumed.

If the thickened portion of the nerve had been removed, and if this thickened comprised also the diseased portion, reunion might have taken place, and the reproduction would most probably have assumed, in consonance with a natural structure, the natural offices of the nerve. If reunion had not taken place, the pain would have been prevented, and the limb in some measure incapacitated. It would not have been totally incapacitated, because the sound nerves

would have been adequate to a considerable function; and the powers of a limb are increased after the division of a nerve, before its reunion has taken place.

Every operation in surgery of any consequence is founded upon no better than the expectation of *a probable result.* We *may* save life by operating for hernia, but the patient may die of peritoneal inflammation; we *may* avert impending death from aneurism by tying an artery, but the vessel might ulcerate: we may thus hasten the event, and should certainly preclude the chance of a spontaneous cure.

The proposition of Mr. Denmark had as reasonable a foundation for hopes as almost any other operation in surgery, if (as indeed is not indicated by the expression "above the wound") it was designed to have removed a diseased structure of the nerve. If the preservation of the limb were desirable, the tumor should have been exposed: this constituted a fair mark for the operation: rather more of the nerve on either side than was occupied by the tumor should have been removed along with it. The probability is, that in time the remaining portions of the nerve being natural, would have produced from their extremities a natural medium of reunion, which would have been followed by a restora-

tion of the function of the nerve. If reunion had not taken place, the faculty of sense in the nerve would have been precluded; and the recurrence of pain would have been no more hazarded than by the removal of the limb. If reunion had taken place, and in consequence of the *disease* having extended further than was denoted by the structure the phenomena of it should have returned, amputation might still have been reserved as a last expedient.

The ganglia of the nerves sometimes appear preternaturally large; but I am acquainted with no cases relative to this system which furnish facts worthy of being mentioned.

Tumors situated in the neighbourhood of a nerve are liable to be mistaken for an enlargement of the substance of the nerve, of which the following is an instance. A woman, in wringing clothes, occasioned a strain of the elbow. A hard elastic tumor formed between the inner condyle of the humerus and the olecranon. The pain arising from this tumor was without intermission; it extended up the arm, and reached the neck; and it prevented effectually the motions of the joint. Any degree of pressure produced an augmentation of the pain, which passed up the arm, and extended down the fore arm, affecting particularly the two last fingers.

It was directed that the skin covering the tumor should be blistered, which was done without any apparent benefit. Tar ointment was next rubbed upon it, under which treatment a considerable reduction took place. About this time I first had an opportunity of seeing the woman: the swelling was not larger than a filbert; the joint had in great measure recovered its freedom of motion, but a slight degree of pressure was still productive of pain.

When the fore arm was bent I could easily pass the tumor over the projection of the condyle; it was not, *when in this situation*, susceptible of pain by any degree of pressure. The locality of the disease was thus identified; for if the tumor had been formed in the substance of the nerve, as was suspected, the same degree of pressure would certainly have produced an equal degree of pain, whatever situation it might be made to assume; indeed the pain would most probably have been much greater, if the tumor was displaced, and compressed upon the condyle, than when treated in the same way in its natural situation. I believe that the direction of the pressure and its consequent effects will always afford a criterion for distinguishing between tumors which are merely contiguous to nerves, and those which occupy their own structure. If pressure be made in such a direction as to

remove the swelling from the course of the nerve, the effects of compression would thus be prevented: but if the tumor were *in the nerve*, the same pressure, whatever the direction of it might be, would equally affect the sensibility which peculiarly belongs to the structure, and which is augmented under this condition of disease.

My idea of the above case was, that an effusion had been produced by the injury of a bursa; that the irritation of continued labour had occasioned its enlargement; and that, having attained a certain size, it compressed and irritated the ulnar nerve; the phenomena of which pressure of course disappeared, when the bulk of the swelling was reduced.

SECTION III.

EFFECTS OF EXTERNAL INJURIES OF NERVES.

ARTICLE I.

Inflammation of Nerves.

THE inflammation of a nerve is a condition of it from which we are induced to expect the most serious consequences. Yet it is, in truth, a state which there is much reason for supposing occurs very frequently, without being accompanied by those terrific circumstances which we are at first thought inclined to apprehend. Without any further remarks in this place, I proceed to offer a few examples of the occurrence.

In all wounds, the nerves, as a principal component of structure, receive a greater or lesser degree of injury: in amputation the injury is

inflicted on the trunks: it therefore appeared desirable to ascertain to what extent its effects proceed, and of what kind those effects are, which may be in this way produced.

With this view, I divided one of the largest of the brachial nerves of a rabbit with a bistoury. Sixteen hours afterwards the animal was killed, and the nerve examined. A distinct inflammation had taken place in the superior extremity, which extended about $\frac{3}{4}$ of an inch: the redness was limited to this portion, and was not perceptible in the slightest degree below the place of division.

A longitudinal incision was made nearly through the substance of the nerve, so as to admit an eversion of its edges; the structure of it was then examined with a powerful lens. The appearance of the centre was slightly red, and evidently of a darker colour than that displayed by a similar incision of a healthy portion. This examination proves that the whole of the neurilema is liable to be pervaded by inflammation. It is probable that the effects would in many respects be the same, if the inflammation were established only in the investing membrane of a nerve, as the local function of it is dependent upon that of the vessels by which it is supplied; and a process, which would in them induce an

increased circulation, must proportionately affect the function of the nerve as far as it is dependent upon a natural relation with its fluids; changing at once its hydraulic connexion, and that which it has with the properties of the blood.

By this experiment, we are led to believe that a slight degree of inflammation in the trunks of nerves is very frequently produced without any ill consequences; the continuance of it is perhaps cotemporary with that of the acute pain which also succeeds to wounds. This is a degree of inflammation of a nerve which may end in resolution.

I am acquainted with no unequivocal examples of the exclusive suppuration of nerves: reasoning from analogy, their liability to this process is no less denoted than that of the other structures where it is found to take place. That an injury of a nerve may induce suppuration in the other textures (though by what mode it will not here be attempted to determine), appears from the following facts.

A fortnight after the application of a ligature to the sciatic nerve of a rabbit, at the distance of an inch and a half from the spine, a small swelling was perceived, in the direct course of the nerve, about two inches below the place of

injury. This swelling gradually increased for the space of two months, when it had attained the size of a walnut: the skin was denuded, highly inflamed, and it ulcerated at three or four points: from these holes a thick curdy matter exuded, but not with sufficient facility to produce any great diminution in the size of the abscess.

The portions of the nerve were still separated; the superior was terminated by a large bulb about the size of a filbert, and the membrane of the nerve was continuous with the investment of the swelling, which latter contained a whitish pulp.* The inferior portion of the nerve (which extended more than an inch above the place of the abscess first described) terminated by adhesion to a fascia, while a little adventitious matter had formed around it, giving it also the appearance of a bulbous extremity.

On another occasion, three weeks after an injury of a sciatic nerve by a ligature, an abscess had formed in the course of the nerve above the

* Whether this pulp may be regarded as the result of a suppurative process, was not ascertained: indeed, though much has been said upon the subject, it is with me a question if we yet possess a certain test of the identity of pus; a test which will distinguish it from every other animal product. The contents of the tumor resembled in appearance the concretion which is formed upon granulating wounds in the same animals; and bore no similitude to the medulla of a healthy nerve.

second joint, and removed from the influence of pressure and collision against the ground, at a distance of an inch and a half below the injury of the nerve. This abscess ulcerated, and discharged its contents. The ulceration extended, so as to destroy the skin to an extent which corresponded with the circumference of the abscess: the wound then granulated, and healed in less than three weeks. The cicatrix remained for some time thick and indurated; the edges became inflamed, and pus was continually exuding from it: in this state it continued until the animal was killed, which was about six weeks, without manifesting any disposition to heal. No unusual appearance was observable in the trunk of the nerve, at the place which corresponded with the situation of the abscess.

The sloughing of nerves is a process which invariably takes place to an irregular extent, after the application of a ligature which is suffered to remain until spontaneously detached. Of this fact we shall find many examples in the course of this inquiry: the introduction of them here need not interrupt the detail, which, besides the endings, respects also some other phenomena of inflammation.

Oct. 9th, I exposed the left sciatic nerve of a rabbit, and tied it with a ligature composed of

two threads, at about the distance of an inch from the place where it escapes from the vertebræ. A general spasm took place immediately, and a great degree of pain was occasioned: this however was only momentary, and the limb was but partially paralysed. On the 10th, the alvine and urinary excretions were discharged involuntarily, and on the 11th he died.

A portion of about three inches of the nerve was removed. It was carefully washed, and on examination I found that a filament of the under part of it had not been included in the ligature: and to this circumstance I attributed the partial continuance of the voluntary powers of the limb. The nerve was in a high state of inflammation, which extended, in the direction downwards, about an inch and a half. The fasciculus of the nerve, which was not included in the ligature, was also inflamed, but the degree was comparatively slight.

In order to ascertain the extent of the inflammation above the ligature, I removed a portion of the spinal column. The nerve was not intimately connected with the medulla spinalis at the point where it emerges from the bones, but ran parallel with it, without any perceptible attachment, for about the space of half an inch. The inflammation of the superior portion was

greater in degree than that of the inferior, and it presented at one place the appearance of a small patch of extravasated blood. The inflammation was greatest just above the ligature: a slight discolouration only was perceptible at the place of the connexion of the nerve with the medulla spinalis; *this last exhibited no sensible evidence of having participated in the process.*

The theca of the nerve was exposed by a careful removal of its looser covering: it was then examined with a lens. The colour of the medulla, exposed by a section of the nerve, was not sensibly changed: the display of inflammation was limited to the membranous structure, and the whole of the neurilema in this situation was pervaded by it.

With a view to ascertain the comparative degrees of inflammation which would supervene upon a partial division of a nerve, and upon the application of a ligature, the left sciatic nerve was tied with a ligature, composed of two threads, about an inch from the vertebral column; the opposite nerve was half divided in the same situation. Both legs were completely paralysed: the alvine and urinary excretions were immediately voided, and continued to be discharged, apparently without the power of retention, up to the time of the death of the animal.

In this instance, as a matter of curiosity, I made an examination of temperatures; of which the following are the results. Atsmosphere, 59; leg, on the nerve of which the ligature had been applied, 67; leg, the nerve of which was partially divided, 69; fore leg, 72. The applications of the thermometer were made near the extremities of the legs, in order that the heat of the body might not be imparted to the instrument; but the varieties were so slight, that I will not attempt to determine whether the results may not have been influenced rather by some accidental circumstances, than by the comparative privation of the influence of the nerves. I am inclined, however, to suspect, as well from these as from other circumstances, that there is a diffused process of calorification in an animal body, which involves a reciprocation of offices between the arteries and the nerves; and that the function which the nerves exercise in this case is but partially dependent on their centres. This opinion has been hinted in the sketch on inflammation, where it is attempted to connect the generation of heat with a property of that principle of animation whose alliance is assigned with the nerves.

Three hours after the operation the animal became convulsed, and in a few minutes died. The nerves were examined in the same manner

as before, and they were both found to be considerably inflamed. The greatest degree of inflammation prevailed in the nerve which had been tied. It extended upwards, as far as the vertebræ; and downwards, about an inch. The inflammation in the nerve which had been partially divided, was not so strongly marked as in the other; but it was very perspicuously displayed, and the extent of it was about the same. A little papilla of medullary matter had exuded from the upper extremity of the cut nerve, a divided fasciculus of which had retracted about two lines.

In order to ascertain as nearly as possible the effect of dividing a nerve for the prevention of morbid actions, which are either to be anticipated or have already taken place in its inferior distribution, I divided one of the largest nerves of the axillary plexus of a rabbit. The section of the nerve produced a general spasm, but the animal did not betray the evidences of pain, so acute, as that which was occasioned by the application of a ligature. The lower portion of the nerve was tied about half an inch below its truncated extremity, and not the slightest indication of pain was evinced. It is abundantly proved by other experiments that the *sensibility* of the lower portion of a nerve is destroyed by intercepting its communication with the brain:

but I was desirous to ascertain whether that which we denominate the *irritability* of a nerve depended upon the continuity of the chord, and the consequent participation of the influence of the brain; or whether the phenomena of it were dependent exclusively upon the properties of its vascular system. I presume there are but few agents capable of producing a higher degree of inflammation than the ligature.

Fifteen hours were suffered to elapse for the access of inflammation: the animal was then killed. The nerve had retracted about two lines: the superior portion of it was slightly inflamed to the extent of half an inch; but no distinct appearance of inflammation was displayed in the inferior portion, either above or below the ligature. The nerve was discoloured; but as the natural complexion of it was immediately restored by ablution (which is not the case when a nerve is *inflamed*), it was fair to presume that the discolouration was imparted by the blood, which was extravasated around it. In this instance the transverse lines of the nerve were more conspicuous in the superior portion than I had hitherto observed them, although the retraction of it was not greater than in other instances.

I have related distinctly these facts relative to the inflammation of nerves, in order that they

might be separately considered: they furnish a copious commentary, which it appeared rather desirable to subjoin, than to intermix in the narration. With the assistance however of these, and of some other data, I shall proceed to offer a few remarks, some of which will be brief, and others will involve a more ample discussion.

The period required for the access of inflammation in a nerve is short: I have found it to be established, in no slight degree, within three hours from the application of a ligature. The first in order of the effects of inflammation of a nerve should furnish the indications by which the state is discriminated. The evidence which relates to this diagnosis is not of a direct and unequivocal kind. A sense of heat and pain arises from those injuries in the human subject, which have been found in brutes to produce inflammation of nerves. We possess therefore the testimony of this connection, as well as that which is derived from some other analogies, tending to evince that heat and pain are to be enumerated among the means of the diagnosis.

We have but few examples of inflammation, which are not attended at some period by sensations more or less painful; this connection is invariable in phlegmonous inflammation, and perhaps always takes place in some degree, or at

some time, in the instances of chronic abscess; yet these latter may proceed for months without exciting a pain, which will be complained of by the subject of the disease. There are indeed times, when no pain is experienced in the lumbar abscess; while we are assured, by a perceptible progress, that suppuration still goes on. It must also be remarked, that redness and vesication may take place in a limb which has lost its sensibility. These facts prove that inflammation may sometimes exist without pain, or that the presence of inflammation will not necessarily produce pain.

In concurrence with the same argument, we frequently find examples of intense pain, without any marks of inflammation; and this is illustrated in a centre of the system which we are specifically considering. I have seen (and many others must have witnessed similar facts) the brain of a lady examined, whose death was preceded by intense pain in the head, and by alternations of epileptic and hysterical affection; with all of which she had been afflicted for years: yet there were in this brain no traces of inflammation, or other sensible marks of disease. Analogous examples may, if required, be multiplied. It appears, therefore, that severe pain, affecting nervous structure, may exist without inflammation; and that certain degrees or kinds of in-

flammation may exist without pain. On the other hand, as a nerve will be found inflamed in brutes, after an injury which in the human subject produces pain, we appear warranted in presuming with considerable reliance upon this connection, and may therefore mention acute pain as one of the symptoms of an inflamed nerve.

It has been found in the instances of puncture of a nerve (as of the cutaneous of the arm) that the sense of injury has occupied principally the remoter distribution of the nerve; that it has been acute at the place of the puncture; that it has proceeded up the arm; and that spasms of the muscles of the arm and neck have taken place in consequence.

It will be found difficult to connect the distant origin of the pain, with the *inflammation*, of the nerve: the manifestation of an injury, remote from the place of the nerve at which it is inflicted, is a common occurrence: we perceive here a relation which has been termed a sympathy; we might as well acknowledge our ignorance by this word, as by any other; we must be careful, however, that it does not pass for an explanation which respects causes. That this is a kind of sympathy which proceeds further than to affect the faculty of sensation, appears from this fact, that either *ulceration* or *sloughing*

of integument frequently occurs in the extremity, while the place of injury of the nerve is at a short distance from its central termination. This sloughing might take place in the toes, while the superior wound, made in the experiment, is granulating or has already healed.*

That the spasms of the muscles furnish no proof of the presence of inflammation of a nerve, is evinced in the histories of tic douloureux; and by the fact, that spasms take place on other occasions (as in epilepsy) without being preceded by pain, which we have assumed as an accompaniment of inflammation of a nerve; and also that a nerve will go through all the progress of inflam-

* The most frequent course of the progress of affection in injuries of the nerves, is from the place of such injury to their extreme filaments. This tendency accords with the progress of inflammation in the mixed structures, which, though deep seated, makes its way to the surface; following the course of the progress of affection in the nerves, upon which I have considered inflammation to be dependent. Mr. Hunter could perceive no clew to the explanation of this circumstance, and the data here exhibited tend only to show its agreement with some other relations. But the progress of affection of nerves is *sometimes* from the injury towards their centre: accordingly, we find that inflammation sometimes observes the same direction of progress, as is exemplified in the following instance: A young man received a thrust of a dirk in the abdomen: the weapon made a partial penetration through the abdominal parietes; and the peritoneum, as was ascertained on dissection, had sustained no injury: in 48 hours afterwards inflammation of the bowels came on, of which he died. The external wound was surrounded by no more than a healthy degree of inflammation.—The same progress of inflammation is not unfrequently exemplified in the punctured wounds of fasciæ; and both instances were remarked in the experiment of three ligatures employed upon the axillary plexus.

mation, to its various endings, without occasioning any thing like spasmodic affection, of the muscles. We find this to be the case not only in brutes, but also in the human subject: in the former, the examples have been premised; in the latter, we need look no further than to the almost obsolete method of tying the whole cord in removing a diseased testicle, in which the spermatic nerves are subjected to the action of the ligature, which is allowed to be spontaneously detached. On the connection between spasm, and sensible injury, we shall hereafter have occasion to remark further.

It does not appear that we have any *one* certain test of the presence of inflammation of a nerve. But we feel justified in assuming its presence by a combination of the following circumstances, or of those among them which are the most characteristic, namely, a sensible injury in the situation of a nerve, acute and continued pain, which is diffused in the distribution of the injured nerve, together with spasm of the muscles: these criteria are assumed upon the foundation that such effects supervene upon injuries of the same kind, as those which we have found to produce visible inflammation of nerves.

A nerve inflames slightly after a simple division; the inflammation which arises in con-

sequence of a puncture, is more strongly marked than the last, and that which results from tying a nerve becomes of the most violent kind of which the nerve is susceptible. A nerve inflames in consequence of the action of a ligature, and is destroyed to a certain extent by the same cause.

This destruction of the nerve is connected with inflammation; but as there is reason to believe that the death of a nerve may take place without inflammation, it remains as a question whether we are in this case to assign the relation of cause and effect, or that only of coincidence or association; whether the death of the nerve is caused by the operation of an inflammatory process, or whether it ensues from a *specific change* in the relation which subsists between the natural properties of the nerve, and the external causes by which these properties are liable to be influenced.

That the death of nerves may take place without being produced by inflammation, appears to be denoted by the gangrene which sometimes succeeds to the application of the cause, in the short space of a few hours, as is illustrated in many cases of surgery, as well as in some examples which have been recorded of the bites of serpents. These facts are not in con-

sonance with the nature of inflammation, as it is elsewhere observed: it is seen to be a process which gradually attains its greatest degree, and continues as violent as it can be imagined for many hours, perhaps days, before it ends in the destruction of those textures, of which the nerves form a principal part. But without resting the argument wholly on this analogy, we shall derive some assistance by comparing the results of experiments before related.

The inferior portion of a divided nerve will not inflame under the action of a ligature, than which I know of no more powerful cause of inflammation. Notwithstanding this incapacity to inflame under these circumstances, I have found a destruction of half an inch of a nerve to take place in the inferior portion on which a ligature had been applied, although the nerve was divided previous to its application. This proof appears of a more direct kind; it requires only, for a perfect satisfaction, to repeat the same processes, and to observe the condition of the nerve under the different stages of the progressive effect of the ligature.

We are by a similar comparison of experiments, before detailed, furnished with two facts, one pathological, and the other belonging to physiology.

1. As a nerve will not inflame below the place where its communication with the centre is intercepted, so must we consider the division of a nerve at a superior point, as a means of subverting the inflammation by which it is occupied at a remoter distribution.

2. As a nerve inflames in a few hours, below a ligature, as well as above it, and will not inflame when the chord is divided, under the action of the same cause, in the same time; and as the ligature displaces a circle of the medulla, and prevents sensation and motion, while the continuity of the neurilema is preserved; we are on these data justified in concluding, that the continuity of the medulla is necessary to sensation and motion, and that the integrity of the neurilema is essential to the display of phenomena which depend upon its vascular system, and are exhibited in the appearances of inflammation. It is probable that this last faculty will be said to be one of irritability; but I do not profess myself to understand any thing by the term; at least, the meaning is not to me very intelligible, and the distinction which it seems to imply, not without the opposition of perceptible occurrence. If therefore the faculty of sensation be resumed in the inferior portion of a divided nerve, it is not in consequence of an increased vascularity, or an increase of the

vessels carrying red blood, (a question which must arise in some histories of disease) but by derivation from sound trunks. That this conclusion is correct, further appears from the result of the following experiment.

June 25th, I applied a ligature on a sciatic nerve three inches from the spine. Sufficient time was allowed for the access of inflammation: another ligature was then applied on a lower portion at the place of inflammation. The animal started; but I believe this arose from the motion, and mechanical disturbance, produced in the superior portion of the nerve; as he expressed no sign of sensation when the nerve was divided between the ligatures, nor could any sensation be excited afterwards by pulling the inferior ligature. The question which applies to the renewal of the function of an intercepted nerve, has been considered under the article of Relations between Nerves.

But as our design ought to be to expose the state of the evidence, and not to deceive ourselves, it is proper in this place to revert to the instances of abscess occurring below the place of the injury of a nerve. In one of the examples alluded to, considerable suppuration of the integument had occurred, and the extremity of the inferior portion of the nerve was found enlarged

and adherent to a fascia. These effects look like those of inflammation: on the other hand, the lower portion of a divided nerve does not inflame under the action of a ligature. We must consider this last result as a direct evidence; we must regard it as demonstrative that a nerve is indisposed for inflammation, where the influence of the centre is prevented, and that if this capacity to inflame be afterwards possessed, it is obtained by a metastasis, which has been denied with respect to the *sensibility* of nerves.

But the fact of the adhesion and enlargement of the extremity of the nerve is no proof that this portion had been affected by inflammation, as it is commonly displayed; for some of those which are usual consequences of inflammation, may take place without their general accompaniments. Thus the sloughing of integument is an effect which is preceded by inflammation; but it may take place by an injury which does not lead to all the appearances of inflammation, as is proved by this fact, that in the experiment before mentioned, in which three ligatures were employed upon the axillary plexus, the limb, although it *mortified*, never regained even its *natural temperature*. In the instance also of the experiment under consideration, if an inflammatory process produced the thickening and adhesion of the structure of the nerve, this inflam-

mation was attended with no pain, as the portion of nerve in which it took place was found to be destitute of sensibility.

The same exclusive affection of distinct properties of a principle, which we have reason to believe is allied with the nerves, may also be exemplified in the formation of pus, the instances of which have been before remarked. Without suffering ourselves to digress any further from sensible testimonies, we will merely recapitulate the facts as far as they are connected with the question.

A ligature on the inferior portion of a divided nerve produces no sensible inflammation, though a longer period should be allowed for it to supervene than is required for its establishment where the continuity of the membrane is preserved. An injury of a lower portion of a nerve may lead to a thickening of its structure, and determine the formation of matter at a remote point, and confer all the accompaniments of inflammation, as redness, heat, pain, &c. upon the integument which is occupied by the abscess.

Whether we are justified in concluding that an injury of a nerve may produce all the appearances and effects of inflammation, by an influence on the filaments which are connected

with sound trunks, while the injured nerve is itself incapable of more than a partial exertion of the properties which are interested in inflammation, is a question which we cannot decide, until the number of our facts be increased. That the actions of the arteries may be variously modified by an injury of the nerves, is in perfect consonance with the connection which has been said to subsist between them, in the physiological remarks which have been offered, under the title of this relation; and that an injury of a nervous trunk may produce its effects specifically upon remote branches, is a propensity which is shewn to be common in this order of structure.

It appeared desirable to ascertain how far the danger arising from injury of a nerve is proportionate to the proximity of the place of injury to the central termination of the nerve.

With this view I tied the sciatic nerves of rabbits at various distances from the spine, with the following results. At any distance which was not greater than an inch from the vertebræ, the ligature produced death in a few hours. The animals for a short time appeared perfectly at ease, and fed &c. as usual; on a sudden they became convulsed, and immediately died. At a distance of an inch and a half from spine or

further, the sciatic nerve may be tied with impunity. In the same manner, the ligature may be applied to any of the nerves of the axillary plexus, without occasioning death.

It was next to be examined, what was the rationale of these different events? On inquiry into this particular, I found that in the instances of a fatal termination, the nerves had inflamed as far as their connection with the medulla spinalis; and, in those in which death did not supervene, the distance between the place of injury and the central termination was greater than the ordinary range of inflammation.

It became next a subject of inquiry, to ascertain whether the extension of inflammation to the medulla spinalis was the cause of death? That this was not a necessary mode of death, was evident from the following considerations. 1st, The inflammation of the nerve had not reached the medulla spinalis; at least this latter was not sensibly affected with it. 2d, The medulla spinalis may itself be subjected to the causes of inflammation, as from a wound, or a fracture of the vertebræ; and the animal, though he will ultimately die, may survive a sufficient time for actual inflammation to have taken place.

As examples of this fact, it may be mentioned, 1st, that we find in the Records of Surgery

the detail of an injury of the following kind; the vertebræ have been fractured and depressed, the lower extremities have been paralysed, and the subject has survived some months. 2d, I have myself divided the medulla spinalis: the animal (a puppy) has lived near an hour, at which time he was killed, and a redness of the cut extremities of the medulla spinalis has been conspicuous; it prevailed the most evidently in the superior portion. Hence it appears, that death may take place when no inflammation of the spinal marrow is to be observed; and that the presence of an actual degree of it is compatible with the existence of life. In concurrence with the same view, it may be further remarked, that an injury of a remote nerve is found to produce convulsive or spasmodic diseases, where the suspicion of the presence of inflammation in the nerve itself is precluded: of this fact we are assured by the examples of tetanus, and by some other histories of diseases which will be subsequently mentioned.

If the death of the animals in the above examples were not produced by the extension of inflammation to the centre of the nerve, it remains to be considered, 1st, Whether the inflammation of the nerve had any share in the event; and, 2d, What is the relation of proximity subsisting between the *places of nerves*, or the different parts of their course, and their centres?

1. The connection between the inflammation of a nerve, and its subsequent effects, is to be considered with respect to the influence of inflammation upon the medulla spinalis, and upon the natural offices of the nerve. As the spinal marrow may be inflamed, or its membrane, (which is the only part of nervous structure capable of displaying vascularity) compatibly with the continuance of life, we must reject the supposition, that death resulted in consequence of a condition, which may take place without such event, and which in the present instance was not perceptible. As the affection of a nervous centre, capable of producing convulsions and death, may take place where there is no reason to suspect the presence of inflammation, we must set it down for a truth, that there is no necessary connection between the inflammation of a nerve, and the subsequent phenomena, which in these cases of injury were found to be associated with it. We have also the further concurrent testimony, that inflammation may occupy a remote nerve in the human subject (as in the instances of tying the spermatic nerves, &c.) without producing an affection of the centre; while, on other occasions, the same cause, operating upon the same nerves, will produce an affection of the centre, terminating in spasm and death. In either of these instances the degree of inflammation must be supposed to be the

same, for in both the ligatures are spontaneously detached.

We find it necessary then to call in the aid of a predisposition, which exists in different nerves, or in the same nerve, at different times; by which, under a certain injury, the effects in question may be accomplished. This predisposition may be casual, as it is sometimes present, and at others absent, in the same nerves; it is also natural and constant in some parts of a nerve, while it is wanting in others, as exemplified in the sciatic nerve.

2. We are hence furnished with a clew to a partial understanding of the different effects which succeed to the employment of the same means on a nerve, at different distances: and we perceive that the relation of proximity consists in a modification of the function of a nerve, which takes place in the different parts of its course, furnishing a predisposition which may be operative by an influence on the central termination. This predisposition may also exist under conditions of the animal which are not strictly natural: in these cases it is *assumed*, not *originally conferred*; and the effects we are considering may then arise from the operation of any adequate cause which may be exerted upon it, although the same cause should at other times fail to produce such effects.

Upon a review of the evidence, we find it demonstrated that injury of a sciatic nerve is dangerous in proportion to its proximity to the medulla spinalis; that the perceptible effect of such injury is to produce inflammation of the nerve; and that the injury is followed by consequences which arise from specific relations, not indispensably requiring the presence of an inflammatory process: this last conclusion is not a direct result, but is supported by an unexceptionable analogy in the same system.

If we take up the investigation from this point, and continue it further; the proper order of inquiry suggests that we should next examine the nature of this specific relation, which is assumed to be a natural one. The relations of connected organs have been before divided into two classes; those of privation, and those of affection; the former being one of dependence, the latter one of communication.

That the disorder of the central termination of nerves, arising from injury of their distributions, is one of communication, appears from this circumstance, namely, that the injury of an extreme branch may produce convulsions, and perhaps death, while these effects will not follow a total abolition of the connection, as by the division of the trunks. We have less diffi-

culty in classing the relation in this, than in any other instance: the conclusion is supported only by an analogy, to which however I can perceive no exception.

From a collective regard of the data, the history of the death of animals, as occasioned by ligatures upon sciatic nerves, appears to be this: inflammation succeeds to the operation of the ligature; a specific function of the nerve is also influenced by the same cause: the condition of the properties involved in this function is communicated to the medulla spinalis; its effect upon the medulla spinalis is to produce another change in properties allied with this chord; which change is again influential on the superior parts of the system, either by communication or by privation; if by the former, subverting the natural state of the properties, by positive influence; if by the latter, preventing that communication of influence upon which the functions of superior organs depend. That the former is the fact, appears to be indicated by the most apparent order of influence in the nervous system, which is from above, downwards. This however is a point which is not clear.

We have seen that the inflammation which succeeds to a puncture of a nerve, is greater than that which arises from a total division; but

not so considerable as that which ensues from the action of the ligature in the same time.

The effects of punctured wounds are of a very singular kind: we find that in the mixed structures they are of a more formidable nature than those of incision; and the same thing happens in the nerves. Spallanzani has remarked, that newts will live many days after decapitation, while a puncture of the cervical medulla immediately kills them.* It is difficult to connect the pathology of punctured wounds with processes of more palpable occurrence: but I presume, *à priori*, that the same pathology belongs to them in the mixed structures, as in the nerves; and that their phenomena take place in the former, in consequence of a primary operation upon the latter.

In the case of puncture of the cervical medulla, it must immediately strike us that there is a double source of derangement; the one, by an influence upon properties of the inferior, and the other upon those of the superior parts of the system: and as the communication

* This is a particular which has been confirmed by other naturalists: and it remains to be reconciled with M. Le Gallois' conclusion, that the faculties of the organs of respiration have a cerebral origin. There is very much more in this matter than has been developed.

between these parts is only partially interrupted, we must conceive that there are two sources of affection, or of influence by communication, tending to establish reactions, and a complication of processes, which may lead to the extinction of connected functions, allied with individual parts. In this way, I am disposed to account for the benefit which is said to be gained by the division of an artery above a ligature, or by the total division of a wounded nerve, rather than by attributing such benefit to the removal of tension, which may be done as effectually by approximating the superior and inferior portions by the flexion of a joint: how far the conjecture is explanatory I leave others to determine.

The surgical treatment of an inflamed nerve, is comprehended in the fulfilment of two indications. The first refers to measures which are dictated by the general principles of surgery: and the second to those of a specific kind, the design of which will be to prevent the possession of the faculties in the inflamed portion, which are essential to the existence of inflammation.

The treatment which is directed by general principles, is comprised in the employment of evaporating lotions, bleeding, saline purgatives, &c. The mode of subverting the inflammation of a nerve which is more peculiar to the organ,

and of preventing the consequences which may arise out of its condition, consists in a division of the nerve.

It will appear of some importance to select a point for the operation, which is above that to which the inflammation has extended. In the course of my experiments I have never observed the inflammation of a nerve to have extended further than an inch and a half, and rarely more than an inch. It would however be a correct practice, to accomplish its division, if possible, at a distance of two inches above the place of the wound.

As the inflammation which requires the interference of surgery, is generally produced by a puncture, it is probable that it would speedily cease, even though the nerve should be divided at a point which is already occupied by disease. It would however be most prudent not to incur the risk of producing a temporary increase of excitement, in a place where the degree of it is already too great. The division of a nerve would speedily subvert the inflammation of its inferior portion, in which we have ascertained that the condition is not excitable by the action of the ligature: it would also prevent the consequences, in which the other properties of the nerve, dependent upon central communication,

would be interested. If however a tendency to that which has been before indicated as a specific affection, should obtain in the superior cut extremity, there is reason to believe that the phenomena which are peculiar to such predisposition would take place.

In the case of inflammation of a nerve, we have no selection of means for intercepting the communication with the centre. The ligature is shown to be inapplicable, by the facts, that it is itself a cause of violent inflammation, and that the inflamed condition of a lower portion of a nerve will take place, where the integrity of the neurilema is preserved. The division, then, by the knife is the only specific mode by which we can reasonably attempt the cure of inflammation of a nerve.*

In the cases of inflammation (arising from puncture) of a cutaneous nerve of the arm, Mr. Abernethy has judiciously recommended the use of a tourniquet, with a view to prevent the obscurity which must arise from the effusion of blood. With this assistance, it does not appear difficult to expose and divide either of these nerves. Their situation for the extent of an

* It is unnecessary to repeat the symptoms of an inflamed nerve: they are discussed in the beginning of this article.

inch above the orifice in the vein (presuming that the nerve is pricked in phlebotomy) will be nearly in a line drawn upwards from the place of the puncture: the deviation of the nerve from this line on either side will follow the convergence of the biceps muscle to its tendon.

If the incision be longitudinal, it will probably be made on one side of the nerve which may then escape detection: if it be transverse, the nerve would perhaps be cut through in the attempt at dissecting down to it, and the operator may continue his incision to an unnecessary depth, from the want of a visible testimony that the object of the operation has been accomplished. The pain occasioned by cutting through the nerve, would indeed furnish some indication of the occurence; but this test would perhaps mislead the operator, inducing him to think that the nerve was divided, while it may be only wounded: nor is it to be relied on in all subjects, for there are some so irritable, as to express an agony which one would think could hardly be exceeded, when they are but little more than scratched by the knife; and there are others so phlegmatic, that all pain is alike to them. I have seen a man inspect the processes of the amputation of his own thigh, with as much *sang froid* and apparent unconcern, as the most indifferent spectator.

The incision which would comprehend both the above advantages, would either follow an oblique line, or it would be curved, so as to form rather less than one third of the circumference of a circle. Such a wound would traverse a considerable space, and would admit a dissection, which is almost longitudinal; thus enabling the operator to expose the nerve to a short extent, previous to its division.

The division of a punctured nerve has received the sanction of practice; on this occasion, it must not be forgotten to acknowledge our obligations to Dr. Monro, for the first demonstration of its utility and success.

It is of importance to ascertain what are those diseases of the nerves which will, or will not, be renewed after a division, when the branches have again acquired the influence which is obtained from the centre, whether by reunion, or in any other way, supposing any other way to be possible. When the lower portion of a diseased nerve is deprived of cerebral influence, it is, in regard to its properties of sense and motion, placed in the same condition as the lower portion of a sound nerve, which has been treated in a similar manner. If therefore the natural function of the nerve be modified upon a restoration of the cerebral influence,

this change, as it does not originate in the superior parts of the nerve, must be referred to a peculiar disposition of that which is occupied by disease. If a nerve have no other relation to its properties but that which subsists between its structure and the cerebral influence, the structure, in all cases of the renewal of the disease, must have undergone some change.

This reasoning appears fair; but it is embarrassed by the facts which prove a function to reside in remote nerves independent of the brain, and which is not productive of sense or motion. If this function be independent of structure, (which I think may be demonstrated, although allied with it) the disposition to the renewal of disease might also be independent of structure.

We cannot therefore, owing to this discord of the data, infer that every disease in the lower portion, which recurs on the restoration of cerebral influence, is one which involves a change of structure in the nerves.

But we have an experience, which will be found to extend to individual applications. This experience teaches us, in the present instance, that a disease of the nerve which is merely inflammatory, may be permanently cured by in-

tercepting its communication with the brain: and it induces us to believe that the diseases of the nerves which are spontaneous and of long establishment, or those arising from injuries which have produced specific effects, perhaps capable of disordering the brain, and not included in the phenomena of simple inflammation, will return upon a restoration of the faculties which may be in any way obtained from a central termination.

ARTICLE II.

Injuries of Nerves producing Spasms.

MY design in the present article is to connect as many facts and considerations, as will serve to form one step towards the elucidation of the *pathology* of tetanus; as well as to subjoin the sequel of some discussions, which have been before hazarded. There is a case in the Philosoph. Transact. for 1801, by Sir E. Home, which is in a great degree illustrative of the tendency of disease in the nerves. This case is amply told; and though the detail is a long one,

it scarcely admits of being abridged without the omission of some essential particular. I must beg pardon for introducing so long a quotation, and nothing but the design of illustrating the subject, in more topics than one, could excuse it to myself; for it is a remark which in the course of my reading has often suggested itself, that if a person has nothing more to write than that which he finds prepared at his hands, he had better not trouble his wits in putting together such disjointed shreds. Presuming therefore upon the sufficiency of this pledge, I proceed to the citation of the case: the history of which, informs us that, " A person thirty-six years of age, naturally eager and anxious in his disposition, whose stomach was peculiarly irritable and irregular in its action, in the winter of the year 1796, while riding in the country, was thrown from his seat by a sudden motion of the horse, and in endeavouring to save himself fell with his whole weight upon the end of his thumb against the pommel of the saddle.

The part swelled and became very painful. A few days after he hurt it again, which prevented the swelling from subsiding, and it remained uneasy and enlarged for three or four months. It afterwards got well, but the motions of the thumb were not always under the command of the will; so that he was sensible in the

years 1797 and 1798, while writing, of finding a difficulty in forming particular letters.

On the the evening of the 16th of Oct. 1799, which was cold and damp, he was travelling in a post chaise with two other persons, and let down the window to speak to the driver. A cold wind blew directly into the carriage, and he endeavoured to pull up the window; but not seeing the glass rise, he looked down, and his hand, instead of pulling up the window, was lying upon his knee. The thumb was bent in towards the palm of the hand: a spasm came upon the muscles of the arm, making them bend the elbow, and immediately he became insensible: in a quarter of an hour he perfectly recovered himself. Some hours after, upon bending his thumb, to show what had happened to him in the carriage, there was a return of the same attack, which also rendered him insensible for a few minutes.

From this time he had no return of these attacks for nine weeks; at the end of which period, on the 18th of December, 1799, he was waving his hand with a degree of eagerness, as a sign for some people to make haste and follow him; this exertion made the thumb contract towards the palm of the hand, and he fell upon the ground in a state of insensibility. This

attack went off as the others had done: he had another in the evening, and in the course of the day two more equally violent. As the motion of the thumb was the first symptom in all these attacks, the assistants were led to contrive a glove, the front of which was strong enough to resist the motions of the thumb, and to keep it in its place; while this was kept on, the attacks were less frequent. A ligature was then applied round the fore arm; when the thumb was beginning to be agitated, this was tightened, and the spasms were found to be arrested at the ligature, and of course deprived of their violence.

From this time, a tourniquet was kept constantly upon the fore arm, and a person was always in readiness to tighten it the moment the spasm was expected, which was always preceded by a general feel of uneasiness all over the body: as soon as the spasm went off, as it did instantaneously, the tourniquet was loosened. The spasms of the thumb and fore arm returned frequently, and at irregular intervals, generally every three hours, sometimes oftener, and once did not come on for thirty-six hours.

On the third or fourth day electricity was tried, with a view to relieve the spasms: sparks drawn from the thumb produced tremors in the

muscles which were confined to the thumb. An electric shock through the ball of the thumb brought on a very severe spasm in the arm; but neither sparks nor a shock through the other thumb produced any sensible effect.

On the 29th of December, I first saw the patient; and, after watching the symptoms for three days, made the following observations upon the complaint.

That the beginning of the attack was some involuntary motion of the thumb and fore finger; and therefore the disease appeared to be in the branch of the nerve which supplies these two parts, called by Winslow the median nerve.

That the progress of the spasms was in the direct course of the trunks of the median nerve, up to the head.

That compressing the parts in the course of that nerve, when it was done before the spasms had reached them, always arrested their progress; but when once the muscles had become convulsed or agitated, the same compression had no effect in stopping the progress of the spasms.

The mode in which the spasms were propagated along the course of the nerves was as follows.

Five or six tremors took place in the flexors of the thumb and fore finger; then similar convulsive motions affected the muscles of the fore arm; soon after the muscles of the arm were thrown into the same kind of action; afterwards the pectoral muscle, and scaleni of the neck: the muscles of the lower jaw were probably in the same state, although their action was not within the notice of the by-standers. The head was pulled forcibly to that side in quick successive motions, and in a second or two the whole ceased; the parts became tranquil, the insensibility went off, and the patient recovered himself: there was however a general feel of langour and distress over the whole body, before the recovery.

From these observations, the disease appeared to be decidedly in the inferior branches of the median nerve; and the irritation was conveyed along its course, from its termination in the thumb and fore finger, to the origin in the brain.

It was proposed to divide the nerve, as it passes from under the annular ligament of the wrist towards the thumb, to cut off the communication between the diseased extremities and the trunk of the nerve, and so put a stop to the progress of irritation which constituted the disease.

That such an operation might be attended with success, was not only rendered probable from reasoning, but the performing it was fully justified by the success which had been experienced from a similar operation, in some cases of *tic douloureux*, a disease in many respects of the same nature with the present.

All these circumstances were explained to the patient, who, from a desire of obtaining relief, consented to have the nerve divided. This was done on the 1st of January, 1800, in the following manner: the nerve, as it passes from under the annular ligament towards the thumb and fore finger, was laid bare for above an inch in length; it was then detached from its lateral connections, and in this exposed state a probe-pointed bistoury was passed behind it, and the nerve was raised upon the edge of the instrument, so as to be distinctly seen by the different medical gentlemen present, before it was cut through. As soon as it was divided, the two cut ends retracted from one another to a considerable distance. This retraction was very unexpected, as the nerve was disengaged from the cellular membrane, and no other part had been divided whose action could make the portions of the nerve recede.

That nerves when divided do retract, is well known in the practice of surgery; but this

effect has been usually attributed to the contraction of the neighbouring parts, as the cellular membrane and blood-vessels, with which the nerves are connected. As none of these causes could produce the effect in the present instance, it was natural to suppose that an independent action existed in the nerve itself, which had been so much increased by the influence of disease, as to become unusually great; and therefore the retraction was more distinctly seen than in a healthy state of the body.

The moment the nerve was divided there was a spasm over the whole body, and a momentary insensibility. The blood-vessels divided in the operation were not secured by ligature, but allowed to stop of themselves, to give the wound every chance of healing by the first intention. The edges of the skin were carefully brought together, and kept in that state by compress and bandage, to promote as much as possible the union.

For eight hours after the operation the parts were perfectly quiet, and there was no spasm. The wound then began to feel hot, as if a red-hot coal had been applied to it. To relieve this sensation, the outer bandage was loosened, and immediately there were twitches in the nerve, which soon went off. The patient felt himself generally unwell, extremely nervous and irritable.

Fifteen hours after the operation he had a violent spasm, which went along the arm to the head, but did not affect the brain. In an hour there was a second attack, at which I was present; the pulse was 105 in a minute, the tongue white, a great deal of general irritation, nervous twitches all over the body, but in the greatest degree in the arm and leg of that side. The stiff-fronted glove was now put on to confine the thumb.

Twenty-four hours, or one day, after the operation, the first dressings were removed: the thumb was much swelled, and no union whatever had taken place; the spasms returned every five hours, but were less violent.

The second day, there was no abatement of the symptoms, but the spasms did not affect the brain; they were not now stopped by the pressure of the tourniquet, as they had been before the operation.

The third day, there was an interval of ten hours between the spasms; and in the night they did not extend beyond the elbow.

The fifth day suppuration took place in the wound; the swelling of the hand was much abated; and the patient was able to dress and

shave without spasm, having only twitches in the fingers and tremors in the fore arm.

The sixth day, there was a burning pain in the hand, and a numbed heavy feel in the thumb and fore finger, similar to what the patient recollected to have felt four years before when he bent his thumb.*

The seventh day, the patient awoke with great pain in the hand, succeeded by violent spasm, which passed up to the head, although the tourniquet had been previously tightened: after this, he had no spasm for sixteen hours.

The eighth day, the hand was less swollen and less painful; and he had only two spasms in twenty-four hours.

The ninth day, the swelling had subsided, and the twitches ceased: in thirty hours there was only one slight spasm, which did not go beyond the wrist.

* This circumstance indicates either that there may be a metastasis in disease, which has been found not to take place in the natural state of the nerves; or that a nerve, under some modifications of disease, may produce the faculties which are otherwise referred to a centre, which has been found not to be the case in simple inflammation of a nerve.—*Note by the Author.*

The sixteenth day, the wound was entirely healed; and as there had been no return of spasms, the patient was considered as well.

On the twenty-fourth day, which was a fortnight after the spasms had ceased, at nine o'clock in the morning, he was awakened by a violent spasm which passed directly up to the head, and affected the brain, producing insensibility; this was the only time the brain had been affected since the operation.

Two days previous to this attack he had a violent diarrhœa, and on the preceding day had undergone unusual fatigue.

The tourniquet, which had been laid aside, was now applied, and for the security two were placed upon the fore-arm and one upon the arm itself. At six in the evening there was another spasm attended by insensibility, although the tourniquets had been tightened. The hand was found swelled as well as the wrist, and the cicatrix formed a hard welt, tender to the touch. This hard state of the cicatrix, in which the end of the divided nerve was included, appeared to be a probable cause of the return of the spasmodic attacks.

The twenty-fifth day, the pulse was 100 in a minute, and every two hours there were slight spasms.

The twenty-sixth day, there were eleven spasms at irregular intervals, in twenty-four hours; eight of them went up as high as the head. As the spasms were not stopped by the tourniquet as before, it was proposed to make the pressure directly upon the nerve: this was done by placing pieces of cork in the course of the nerve, and confining them there by the band of the tourniquet, so that when the screw was tightened the cork was pressed down on the nerve. This pressure gave great pain, and instead of arresting the progress of the spasms seemed rather to increase their violence; it was therefore left off.

The twenty-seventh day, the pulse was only between 80 and 90 in a minute; there were seven spasms, all of which were arrested by the first or second tourniquet.

The spasms went on with very little variation till the thirty-ninth day at six o'clock in the morning, when he was seized in his sleep with a violent spasm attended with insensibility, and convulsions over the whole body: these lasted for twenty minutes. After his recovery the

hand was found much swollen, and the welt formed by the cicatrix was painful. In the course of the forenoon he was well enough to bear going out in the carriage, the fresh air always proving very grateful to him.

From this time the swelling of the hand and the hardness of the welt diminished; and the spasms were less violent and seldomer. On the forty-fifth day there was only one slight spasm in 26 hours. In this state he went into the country; and for the first fortnight the spasms diminished, but afterwards became more violent.

The return of the spasms after the wound had been healed made it evident that the operation of dividing the nerve had not answered the purpose which was expected from it. The failure probably arose from the wound not healing by the first intention: the consequent inflammation rendered the cut end of the nerve uncommonly irritable; and in this state the confinement in the hard thickened cicatrix, rendered it liable to be stretched by every motion of the thumb, so as to bring on spasmodic contractions.

From this time the patient was not under my direction; but I understood that he tried the

effect of large doses of opium, which did not afford relief. He was then induced to employ electricity, which was also unsuccessful; and he died in a fit, which at the time was believed to be apoplexy, about five months after the operation had been performed; but as the body was not examined, the nature of the fit could not be ascertained.

In this case some of the branches of the median nerve had acquired from disease an unnatural power of contraction, which was made evident by the operation; and there is every reason to believe that the spasmodic attacks which took place were in reality convulsive motions in the nerves themselves, which excited corresponding contractions in those muscles which were under their influence."*

These are the principal facts belonging to this interesting case, to which I shall have occasion to revert. A few alterations are made from the original, for the purposes of abridgement; but I trust that in taking this liberty I have omitted no essential circumstance.

* Such contractions are at most but the *effects* of a disordered condition of the nerve. That the contractions of the muscles were not *correspondent* with the contractions of the nerves, is to be presumed from the absence of any example of a contraction of a nerve, which exceeds $\frac{7}{8}$ of an inch.

The following cases constitute a remarkable coincidence; it is to be regretted, on some accounts, that the report, which I give as it was transmitted to me, is not a little more minute in the detail.

"May 27th, 1811. Wm. Ryley was admitted into the Birmingham Hospital about a fortnight since, having met with an accident which fractured the fibula of the *right leg.* He kept his bed, and went on well until this morning; when he arose, walked to the fire, and behaved in a very strange manner. On his return to his bed he became affected with hemiplegia of the *right side:* violent convulsions succeeded, which ended with his life at six o'clock in the evening. The following morning the brain was examined.

"The dura mater was much indurated in many places; on cutting into the right hemisphere, a tumour about the size of a pigeon's egg presented itself. It lay imbedded in a great quantity of pus, such as is found in scrophulous swellings. The tumour was of a firm nature, and consisted of a cluster of (perhaps four or five) hydatids.

"In the brain of a patient who diedon the following day, a similar abscess was discovered, but containing a greater quantity of pus: there was however in this case no tumour. This man

had undergone amputation of the *left leg.* He was going on favourably until about two days before his death, when he became affected with hemiplegia of the *left side.* He placed his hand frequently to his head and appeared to suffer great pain in this part."

These cases furnish a singular coincidence; but I do not perceive that they admit of any inference which assigns a connected causation. To suppose that the injury of the nerves of the leg, in either case, was productive of the suppuration in the brain, would be to admit an effect which is not supported by the general results of the most frequent of all injuries; nor can we conceive that the atmosphere of the hospital had any share in establishing a mutual predisposition, which would lead to the formation of an abscess under the operation of such an exciting cause. The facts however are worthy to be remarked.

The attacks of epilepsy have been found sometimes to commence by a painful sensation, a sense of heat, or a sense of cold, at a remote part of the body, and to proceed from thence rapidly to the head: epileptic convulsions have then followed. A tourniquet has been applied between the place of the sensation which preceded the attack, and the superior parts, by the pressure of which the fit has been prevented.

The point of origin has been known to be a tumour in the cellular substance; this tumour has been removed, and the disease has never recurred. The action of a depressed portion of the cranium has occasioned epilepsy, and the disease has been cured by trephining four years after the accident.

We come now to speak of tetanus, the nature of which will be best described by a few examples. The following is a case of trismus, which occurred without being preceded by a wound.

A woman, who had been standing in the street about an hour in an intensely cold day (in the winter of 1813-14), was seized on a sudden with a torpor and incapacity of the whole body: she was perfectly sensible, but she was unable either to move or to speak. She was taken into a house, and made warm by a fire; and in about an hour the motions of the limbs were restored.

At this time I saw her, and found the jaw so closely locked that it appeared impossible to introduce a sixpence between her teeth. I directed the face and neck to be rubbed with a stimulating embrocation, and that an injection should be administered. The pulse was quite natural.*

* Dr. Parry makes the following important remark on the pulse in tetanus: "If, in an adult, the pulse by the fourth or fifth day does not reach 100 or perhaps 110 beats in a minute, I

Two hours afterwards I saw her again; and, by using some force, was enabled to introduce the handle of a spoon between her teeth, and in this way, by a little mechanical violence, succeeded in opening her mouth so as to give her six grains of calomel and a cathartic draught. As soon as the spoon was withdrawn, the jaw became again immoveably fixed. I left her for the night, merely ordering the injections to be repeated every three or four hours.

My reason for directing the treatment so specifically to the bowels was, that, as far as I could understand from those who lived with her, she had had no alvine evacuation for six or seven days. In this supposed connection between the state of the bowels (establishing perhaps the predisposition) and the symptoms I was not deceived.

On the following morning my patient was in the same state as on the preceding night. I was able, though not without much difficulty, again to open her mouth by the same means, and to give her more cathartic medicine. This operated in about two hours afterwards, and pro-

believe the patient almost always recovers. If, on the other hand, the pulse on the first day is 120 or more in a minute, few instances will, I apprehend, be found in which he will not die." Cases of Tetanus and Rabies Contagiosa, p. 18.

duced very copious discharges from the bowels. In less than an hour after the first effect of the cathartics, she was able to open her mouth, and to talk intelligibly; though, before this effect, not the slightest abatement of the spasm had taken place. A little stiffness remained about the neck, which gradually left her.

A waggoner abraded the skin on the back of the hand for about the extent of half an inch. The injury was superficial, *no tendon was exposed*, and the wound healed in about a week. A few days after, the man complained of stiffness of the muscles of the neck: the jaw was speedily locked, a state of rigid spasm affected the whole system of the voluntary muscles, and the complete state of tetanus was thus established, in in less than 24 hours from the notice of his first symptoms. There was scarcely time for the trial of remedies, assistance not being immediately procured; some of those, however, which have been recommended (and they are all perhaps equally efficient) were enforced: and the man died in about 48 hours from the commencement of the spasm.

A man's leg was grazed by a cart-wheel just above the ancle. The exposure was *superficial*, though the contusion was severe. The wound became sloughy; and the state of tetanus was

rapidly established. In this case, wine and opium were given in large quantities; and blisters were applied on the back of the neck and between the shoulders. Finally, there was not a voluntary muscle in the whole body which was not affected with spasm, and the agony of the symptoms in their progress was intense. On the night of the third day from the commencement of the attack, it was directed that this man should be put into the warm bath; he died on the same night, very speedily after his removal from thence. The disease had then attained its greatest degree, and it is most probable that the warm bath neither accelerated nor retarded its finale. It may be added, however, that De Haen mentions the immediate death of a tetanic patient, on his being taken from the warm bath, and the same occurrence has been noticed by others.

Mr. Dallas thus concludes a detail of cases of tetanus transmitted from the island of St. Vincents: "I have since seen four cases of tetanus; one of which was treated in the same manner as my two unfortunate patients (by mercury and opium), recourse was had to bark and the *cold bath* in the other three; but the same unfortunate issue attended the whole. I have conversed with many eminent practitioners of this and the neighbouring islands, upon the

subject; I am well convinced that there is not one well authenticated case in fifty, where the disorder was brought on by a lesion of the nerve, in which the patient recovered. I speak from what I have myself seen."*

Of the cases of tetanus, an immense number have been recorded: they furnish the materials of a copious compilation, which would serve only to exhibit a striking picture of the disease. In these histories it would be perceived that no remedy which has been supported by the faintest analogy, has been neglected: the event has been alike fatal under all. It is true, that the means which have failed in subsequent trials, have been said to succeed by those who proposed them: it must also be remembered, that the disease has sometimes terminated favourably where no efficient means have been enforced. I have heard of such cases; and one of trismus has fallen under my own notice, on which all the probable resources of the pharmacopcia were exhausted without effect. The case became chronic, and was abandoned in despair: eventually, the patient recovered, when all medical treatment had been for some time discontinued.

The following is a partial list of the means which have been employed for the cure of te-

* Duncan's Annals of Medicine, 1798.

tanus. Bleeding, moderately and immoderately; blisters, sudorifics, the warm bath, the cold bath and cold effusion: cold has also been applied to the body by bandages wetted with æther; opium in all doses,* camphor, musk to more than the amount of a hundred grains in a few hours, ammonia, assafœtida, acids, alkalis; mercury in inordinate quantities, rubbed into the surface and taken into the stomach, so as to produce salivation in 24 hours: wine, brandy, and electricity have been used to stimulate, and no article in the list of narcotics has been omitted to diminish excitement.†

The wound has been soothed by poultices and fomentations, and its surface has been destroyed by caustics. The limb has been suffered to remain, and the limb has been amputated. Patients, it is said, have been cured by all these means; they cannot be cured by any of them; and, finally, they sometimes recover without any treatment at all. We must conclude that the

* A scruple of solid opium has been given every hour, and a dram as a night dose: opium, when given in this way, has been found in the stomach after death in a quantity sufficient to have killed the patient if he had recovered from the disease.

† Mr. T. Duncan, of Grenada, has lately published a case of tetanus, which recovered under the use of tobacco injections: the pulse in this case never exceeded 94. (See note, p. 214.) The case alluded to is published in No. XLII. of the Edin. Med. and Surg. Journal.

worst forms of tetanus are not curable by any known remedy; and that, of the intermediate ones, those appear to recover under every treatment, which would otherwise get well without the interference of art.

The treatment of tetanus by cold immersion, laudanum, and wine, as introduced by Dr. Currie, of Liverpool, is that which is generally considered the best. The immersion in cold water has been said, when it fails to cure the disease, to produce an abatement of the severity of the spasms. Other cases are reported, in which the cold immersion has been prejudicial, increasing rather than relaxing the rigidity of the muscles.*

The forms of tetanus are enumerated under the titles of trismus, emposthotonos, episthotonos, pleurosthotonos, &c.; for my own part, I consider these as no better than medical witticisms, and should be as well contented to acknowledge my ignorance in my own language.

Enough has been said for the purpose of exhibiting the general character and tendency of tetanus: it remains that we should next examine how far the facts we possess are suf-

* Vid. Cases of Tetanus and Rabies Contagiosa, p. 4. by Dr. Parry.

ficient to indicate, in any degree, the order of minute pathological occurrence.

It does not appear to be clearly ascertained on what constituent of structure the primary influence is exerted which subsequently produces tetanus. The disease has been referred to wounds of tendons and to wounds of nerves. We cannot expect to understand how tetanus is induced, unless we are able to define, with tolerable accuracy, the order of parts which is interested in its establishment. It is therefore proper, that our inquiry should commence with this examination. We shall find the argument perplexed by a parity of reasoning, which must to a certain extent be applicable to both structures.

When tendons are wounded, nerves also are injured: if tetanus should supervene to the accident, we cannot, owing to this complication, affirm that it results from the injury of a tendon.

The rupture of the tendo capillis is not an uncommon accident: two or three cases have fallen under my own observation; and one, in which it was completely divided by a scythe. These cases terminated successfully, and I have never heard of this kind of accident having been followed by tetanus. Tendons are also cut with

impunity in amputations about the foot, as of toes and metatarsal bones. They are also punctured by various accidents, sometimes penetrated quite through; and they are lacerated by machinery of various kinds. Of these last accidents, many have fallen under my notice and care. A hand or a foot gets entangled in a wheel, and the subject is fortunate if he escape with the loss only of a finger or a toe: I have seen nearly one half of a foot torn away in this manner, while several tendons have been left hanging from the wound. Tendons thus injured are sometimes cut off, and at others a spontaneous detachment is suffered to take place by sloughing.* Such accidents in general terminate favourably.

But it will be said, these are injuries which involve lesions of nerves, as well as of tendons; we are therefore, by such histories, furnished with no proof of the identity of structure which is operated on for the establishment of tetanus: the truth of this remark cannot be disputed: we must therefore look for other facts which apply more pertinently to the question.

* This latter is the safest mode of detachment. As tetanus has been known to follow the removal of a sloughing tendon by the knife or scissars, while the wound was in other respects going on well; it is perhaps a correct principle, never to attempt the removal of a dead tendon, lest a sound part should be cut, and a disease produced, to which there perhaps existed before a strong predisposition.

In the two examples of tetanus before related, no tendon was exposed, much less injured: in the first, the abrasion of the skin was quite superficial, no pain existed about the place of the wound, which was cicatrized at the time of the supervention of tetanus. Injuries therefore are followed by tetanus, where there is no reason to presume that the tendons are in any degree affected by the cause: but no injury of a tendon can be inflicted, which does not involve an injury of nerves. This conclusion will be admitted when it is remembered that the filaments of nerves (which have properties in common with their trunks) pervade every part of the body. If then the selection lie between the tendons and the nerves, we must acknowledge that tetanus may succeed to injury of the latter, while the former are exempt; and that we possess no evidence which proves that it may arise from an injury of the former, because in the same injury the latter are liable to participate.

We must now revert to our physiology, and compare the symptoms with the relation which has been assigned between the muscles (of which the tendons form a part) and the nerves. The division of the nerves of a limb incapacitates the muscles for contraction, which is an essential character of tetanus: by the influence of the nerves the muscles are capable of contracting,

and it is upon this capacity, conferred by the nerves, that all agents operate, whose effect is to produce contraction. Recurring to the distinctions of Bichât, it will perhaps be said that a voluntary muscle possesses an *organic contractility*, which is independent of the nerves. Of the extent of this faculty I am but indifferently informed from the observations of others; from my own, I am disposed to think it not very great, or almost to consider it of questionable existence. On wounding a muscle after the division of nerves, I have remarked a slightly tremulous motion, which is very different from those forcible actions of the muscular fibres, which take place during the integrity of the nerves: and even these instances of slight contraction I have remarked not to take place from the same cause, at a distant period from the division of nerves. Further, I have offered reasons, which remain to be refuted, for considering every action which takes place in an animal body to be performed by a function which belongs to the nerves; that the phenomena which are arranged under the departments of the animal, and the organic life, are dependent, the first, wholly upon the nerves and their central influence, and the second, to a great extent upon the nerves themselves.

But even granting that a voluntary muscle possesses this faculty of contraction, and in a

considerable degree, it is contrary to analogy to suppose it to be interested in tetanus: for we find on other occasions that the spasms of the voluntary muscles, which acknowledge no controul of the will, are prevented by a division of nerves. Considering therefore that the above conclusion is better sanctioned than any other by the facts we at present possess, the only question which arises as to the identity of the structure which is primarily affected in tetanus, respects the order of nerves, whether trunks, cutaneous, or muscular, filaments? for it is to me evident, as well from the considerations expressed as from those which remain to be adduced, that the relation of the cause which produces tetanus is with the properties of the nerves, and with those properties which depend upon central communication.

As we have seen that tetanus may arise from injury of cutaneous filaments, it is fair to presume that it may arise from injury of fibrillæ in any other animal component; and we are disposed to infer the presence of these fibrillæ in any order of structure, which in any state is capable of sensation, even though dissection should not display them. But this is a particular of no importance; for if the properties of nerves are diffused further than the sensible organs, the same relation subsists between the properties so

diffused, and the trunks of nerves, as if the connection were also preserved by continuity of nervous structure: this is clear by the necessity of reciprocal influence between such parts (if such are supposed) and the nervous centres. It is by these facts to me satisfactorily clear, that the process which leads to tetanus takes place by a change which is most probably of a direct kind, induced in the properties of nerves: and we will therefore, for the sake of a more palpable reference, speak of the operation of injuries as upon the nerves, in whatever way their properties might become affected. The nature of this change comes next to be examined.

That tetanus is not produced by inflammation of a nerve, and is not even connected with it, appears from the following circumstances.

1st, It has been shewn that inflammation of a nerve does not of itself produce specific effects, which are displayed in the phenomena of convulsive affections.

2d, That tetanus sometimes supervenes upon wounds in which there is neither pain nor irritation; nay, even upon wounds which have healed, and the cicatrices of which are in no degree painful. This test has been regarded as a sufficient proof of the absence of inflammation of a nerve, under that article.

3d, The nerves of tetanic patients have been examined after death from the place of injury to their central termination, and no inflammation has been observable in any part of their course. We must therefore reject the supposition of an inflamed nerve as the cause of tetanus.

It has been frequently glanced at in the preceding pages, and demonstrated in that part of the subject of inflammation which treats of the relation of proximity, &c. that the nerves are furnished with latent properties; or else, that the properties which are displayed in their phenomena, are liable to be modified, so as scarcely to be recognised by any analogy, under the influence of some of the numerous causes which affect them.

These latent properties obtain variously in the parts of the system; and they are the agents of a relation which has been termed sympathetic.* They have been shewn to be of two kinds, natural and constant, and acquired or temporary.

* The word sympathy, as implied by its derivation (συμπασχω, to suffer with), is applied only to denote *effects*. It is meant to express connected occurrence; as a testicle is inflamed by sympathy with the urethra, or, in other words, there are strictures in the urethra, and the testicle swells: the word is not indicative of a *process*, or a *mode*: and it is used in medicine as it may be used in the case of a man who is killed by a cannon-shot, namely, that the man sympathized with the cannon; the agents not being glanced at in either instance.

A relation of the former kind has been shown to exist in the sciatic nerve at a distance of half an inch from the spine; and an example of the latter would be found in the remote parts of the same nerve, provided such parts should, as an anomaly, display the same phenomena as those which result from the application of the same cause at a point which is contiguous with the medulla spinalis. Examples, bearing some analogy to both of these, are almost infinite in an animal body. The connection between the parts which are thus circumstanced has been called a specific relation, and the changes which are induced by external causes, specific effects; and we must continue to use this language until we are informed of the agents involved, or have reduced them to a similitude with some more familiar acquaintance.

When nerves *assume* a relation of properties which is thus designated, such state of the nerves may be called a predisposition to specific effects, the history of which it is perhaps impossible to trace, as has been hinted in speaking of the origin of spontaneous diseases. The condition then which the nerve has assumed, and which is followed by tetanus, is one which is independent of inflammation, and consists in a modification of the properties of the nerve. This modification is liable to two relations; one which was natural,

and pre-existent; and another which arises out of the change of identity, which has been assumed in one part of a circle of connections. It is upon this principle (which is familiar to us in our common observations of causes) that the sympathies are exhibited in disease, where no natural relation is shown during health.

We are thus arrived so far as to have ascertained a local change in the properties of a nerve: as this change exerts its influence in no less a field than the whole system of animal muscularity, it is next to be examined whether this influence is direct or mediate; by action or reaction; and what is the course of its progress?

That the influence of the local condition of the nerve is first upon its centre, and that the subsequent effects are produced by a condition of the centre, which takes place in consequence of the operation of the local cause; or that these effects do not take place by an action which is propagated from the place of injury, but by a reaction of a nervous centre; all appear to be demonstrated by this fact, namely, that the muscles to which the injured nerve is distributed, may be among the last to be affected with the tetanic spasm.* I have myself had opportunities

* This inference is liable to be affected by the result of an examination of the modes of that which is termed sympathy;

of observing the fact: tetanus in both the preceding cases commenced by producing spasmodic contraction of the muscles of the neck. In the first case, the wound was in the hand; in the second, in the lower part of the leg. As a question influencing practice, it is of no importance; it is sufficient that the origin of spasm is not at the place of the wound.

This proof appears rather of a direct kind, and it is one of great consequence; but we derive also some testimony from analogy; we obtain it from those cases of convulsive diseases whose origin is in the brain, as in the examples of epilepsy, and in that particular one which has been before mentioned, in which epilepsy was cured by trephining a depressed portion of the cranium. This case was published by Mr. Cootes, of Salisbury: the difference between epilepsy and tetanus, in regard to the conditions of the muscles, is indeed very considerable, but they agree in being both of them modifications of the same faculty of contraction.

If any analogy be perceived in contemplating the immediate affections of the brain, it will not

of which, both in health and disease, there are four kinds, namely, direct, mediate, by communication, and by privation. The result of this examination, as far as it is perceived by me, is in favour of the conclusion.

be found totally wanting in the instances of remote origin: a fit of epilepsy has been preceded by a peculiar sensation in some distant part of the body, it has proceeded from thence to the brain, and convulsions have then taken place. There is reason to believe also that many of the cases of idiopathic tetanus have been produced by causes which operate primarily upon the brain: here again we perceive an analogy; for tetanus, and spasmodic affections, which do not correspond with the definition of tetanus, are both liable to two origins. An instance of disease of this latter description, originating remotely, is furnished in the case recorded by Sir E. Home, as well as by those examples of epilepsy which have been before adverted to.

Assuming then, principally from the fact first mentioned, that a peculiar condition of a nerve takes place in consequence of an injury, which condition produces an effect upon the brain, and occasions a state of this latter, which gives rise to the diffused spasm, and the symptoms which constitute tetanus; assuming that all these premises have been fully sanctioned, it is next to be inquired, what is the nature of the affection of the brain;* whether it is main-

* It would be more correct to say *the central termination of nerves*, as we cannot affirm whether the immediate organ which induces tetanus belongs to the brain or the medulla spinalis.

tained by the presence of disease of a nerve, or whether the condition it has adopted has become one *sui generis*, and independent of the state of the nerve by which it was primitively induced? This question is one which is closely allied with the curative indications. The evidence which should here determine our judgment is short of demonstration; we can reason upon the subject only by the supposition of analogies: let us however examine the evidence as it stands.

The limb has been amputated for the cure of tetanus, the disease has nevertheless proceeded to a fatal termination. But this fact is no proof that the disease is independently maintained by the condition which the brain has assumed; because we know that a disease of a nerve, originally occupying only a defined spot, may extend from thence so as to pervade a considerable portion of the chord. That this was the case in the instance related by Sir E. Home, there can be little doubt; and the same progressive affection is also exemplified in tic douloureux. If the division of a nerve, for the cure of a central disorder, were made at a point which is occupied by disease, although above the first seat of it, it is reasonable to expect that the superior portion, retaining its morbid condition, would continue to produce the same specific effects. We are assured by experience that the operation

for tic douloureux would rarely succeed, if that branch only were divided which was originally occupied by the disease.

The permanence of the spasm in tetanus is no proof that the disorder of the nervous centre independently maintains the disease; for continued action is no more a natural character of the function of the brain, than it is of that of the remote nerves. We perceive also, in many of the examples of sympathy, that the secondary effect, although it may otherwise continue for a time which is not defined, ceases upon the removal of the original cause. Thus a pain in the head is sometimes cured by an emetic or a purgative; thus the breasts become flaccid, when the intercourse of vitality between the mother and the fœtus has ceased; thus a tourniquet (or still more effectually a division) arrests the progress of disorder through a nerve, and prevents the affection of the centre; thus the pain in the shoulder ceases, when the inflammation of the liver has subsided; thus vomiting ceases, when the irritation of an ureter has passed away; thus the swelling of a testicle might be cured by the application of means to the urethra, which diminish irritation, or remove strictures.

The same fact is observable in almost all the instances of sympathetic affections; but it *might*

be otherwise. The independence of a secondary affection is generally established through the medium of a predisposition, which should not belong to the state of perfect health. The disease which takes place in a testicle in consequence of stricture of the urethra, may at first be one comprised in a slow process of inflammation: it is easy to foresee, that if the part be predisposed to schirrhus, the testicle would assume a disease which would remain incurable, though the original cause should be removed: similar in this respect to what happens in mammary tumors, some of which continue for years, and eventually disappear spontaneously, or by very slight means; and others, at first of no specific kind, become cancerous. These gradations of diseases, arising whether from idiosyncracy, or from casual predisposition, are familiar to every surgeon.

In applying these remarks to tetanus, it must be allowed that the most frequent examples are in favour of the position which considers tetanus to be maintained by communication of the disorder of a remote nerve. It will be asked, is there not a predisposition to the disease, as in the instance of schirrhus above quoted? It must be replied that there is a predisposition: for injuries of the same kind, or of greater severity, happening in others, or in the same subject,

are not at other times followed by the same effects. But although it is necessary to assume the presence of such predisposition, the facts afford no room for alleging the structure with which it is allied, to be rather that of the central terminations than of the nerves themselves.

We have seen that amputation, for the cure of tetanus, has failed; but we have excepted against this proof, on the ground that on other occasions the disease, which originally occupies the extremity, may afterwards pervade a considerable portion of the nerve. Have we, it will be asked, any example of the successful treatment of tetanus by amputation? It must be replied, we are not without such an example. Dr. J. Silvester reports a case, in which trismus supervened upon the laceration of the first joint of the fourth finger of the right hand.* In this case, the finger was amputated at the place of injury, which procured temporary ease. Violent pain, convulsion, and locked jaw, succeeded; and this affection was to be cured, it is affirmed, only by the removal of the cause to which it was referred, namely, a lacerated tendon: the finger was therefore amputated at the *second joint;* the symptoms were relieved, and the case terminated favourably. The event of this case

* Med. Observations and Inquiries, vol i. p. 1.

is somewhat illustrative of the common dependence which we have supposed to belong to sympathetic affections; and it indicates, among other considerations which have been expressed, that the disorder of the centre will cease if the nerve is intercepted above the seat of the disease, the extension of which is on some other occasions progressive.

As the establishment of tetanus is shown by these remarks to take place by no single process of disease, such as the application of a cause to a body, the tendencies of which are those only which belong to health, but by the operation of such cause upon a preceding deviation from health, which may be latent until excited, and constitutes the predisposition; and as there is reason to believe that the point of origin still maintains the central affection; we are hence furnished with the following indications of cure. 1st, To subvert the action of the cause which maintains the predisposition (this refers also to idiopathic tetanus). 2d, To prevent the influence of the conjoined agency, upon the central terminations of nerves.

1. The causes of predisposition (except when they are external) are always of an obscure kind: how far they may in the present instance be connected with disorder of the abdo-

minal viscera, or with the presence of *feculencies* in the intestines, capable of originating tendencies to specific disease, by the influence they exert upon the other parts of the system, is a question which is to be determined only by future experience and observation.

That a striking connection obtains between the condition of the bowels and one of the forms of tetanus, has been shown by the preceding case of trismus; which continued in a degree which was unremitting for many hours, and ceased when alvine discharges (of a bad kind) had been freely procured.* If we are allowed to presume upon an analogy, which indeed is not perfect in its parts, the exhibition of purgatives in tetanus is thus indicated: and the view with which they should be administered, must comprehend the design of procuring not one or two evacuations, but of producing a strong and repeated effect upon the primæ viæ, and of making rather a complete, than a partial evacuation.

It is not unreasonable to expect but this plan would operate beneficially upon another princi-

* It is much to be lamented, that, notwithstanding the seeming progress of our art, and the multiplicity of detached remarks on the subject, we have never yet been presented with the faintest resemblance of a scientific view of the natural or pathological relations of the abdominal viscera.

ple, namely, it would tend to produce a *change* in the diffused principle, which animates the parts of a living body: this is in truth a design, on which, for want of one which is more definite, we are frequently compelled to act; and however vague the design might be, and however equal on some occasions the chances might appear of producing an injurious or a beneficial change, our efforts, directed with this view, are not unfrequently favoured with success. These are particulars which are to be known by a development of the sympathies, according to the modes which have been before designated, and by a recollection that the varieties of the principle of animation are those as well of *kind* as of *degree;* that the knowledge of the former is the most diligently to be sought after; and that an exclusive regard of the latter has led to nine tenths of the absurdities which have characterized the modern progress of medicine.

2. The mode of subverting the central affection is by an operation of surgery, which has been performed without success, though not with the same event in every instance: it consists in the amputation of the limb which has sustained the first injury. The amputation of a limb occasions a loss, which, in the event of recovery, would be regretted for life; and the indication does not strictly require its perform-

ance, provided the same end may be accomplished by the exclusive division of the nerves which are interested in the disease.*

As there is no perceptible inflammation of a nerve in tetanus, and as the precise condition of it is to be ascertained by no sensible testimonies, we are furnished with no mode of discriminating the extent to which this condition prevails; a tourniquet may perhaps lend some assistance; but the pressure it exerts is never sufficient to prevent all the faculties of a nerve, some of which may continue, though the integrity of the medulla should be interrupted.

In consonance with the preceding views, it would therefore be proper to accomplish the division of the nerves (whether exclusively, or by amputation) at *the highest attainable point*, as soon as unequivocal symptoms shall have taken place. In a case, so clearly referable to a local condition of the nerve, as that which is related by Sir E. Home, the same principle suggests, that if the disease did not cease upon a division of the nerve near the place of injury, the same should

* This question is to be considered with respect to the probability of a reunion of nerves, and a consequent return of disease: on some other accounts, it would also appear most effectual to remove altogether the injured limb; if this, in a few cases, should be found to succeed, the exclusive division of nerves would probably form a later improvement in practice.

be repeated at a superior point. In conjunction with the attempts of operative surgery, the exhibition of purgatives might also be enforced.

If it be inquired upon what these recommendations are founded? I reply, upon a speculation. I would ask, in return, upon what sort of conviction the great bulk of the actions of our lives, nay, of the most important among them, are undertaken? It must still be answered upon a speculation. In speculations which are related, however distantly, with the experience of every day, we are seldom deceived: thus we are assured that we must die, not because we have the *proof* of it, having hitherto lived; but because we see others die, between whom and ourselves there is an analogy in so many respects, that we presume upon it with the strength of a certain assurance. It is otherwise with the speculations of science: the most essential particulars of the analogy upon which we should found our opinion are perhaps not known; if therefore we suffer ourselves to form an opinion, we do it upon an imperfect estimate of the points of similitude.

It may be expected that the subject of paralysis is one which would with propriety find a place among these pages; that it would be spoken of in the several forms of apoplexy,

hemiplegia, and amaurosis; as occuring from anomalous disease, from poisons, and from external injuries, whether of the head, spine, or nerves. It may also be expected that something should be said of those curious cases recorded under the titles of nyctalopia, hemeralopia, and catalepsy; and that under the head of spasmodic diseases, chorea, epilepsy, hysteria, &c. should not have been omitted. It will however be perceived that these are subjects which are foreign to the plan of this volume; that it was necessary to prescribe some limits to the subject, which in the immense range of pathology is naturally one almost without bounds; and that the proper limitation was that which would comprise only the surgery of the nerves.

On the subject of paralysis, a few remarks have been occasionally offered when the circumstance has been connected with ocular testimonies. I would observe further, that although the occurrence is so very common, I believe there are still some questions relating to it which remain to be elucidated; among which are the following.

What is the reason that one person who has sustained an attack of sanguineous apoplexy (proved to have been such by dissection after death at a remote period) may, in a few hours,

recover from all the symptoms of paralysis; and that another, who has sustained an attack to all appearances similar, shall remain more or less disabled for life? Will it be said that absorption takes place in one case and not in the other? Or will it be said that the brain accommodates itself to the pressure in one case, and not in the other? Or will it be replied that the difference of result arises from the difference of the parts of the brain on which the fluid is effused? I would then ask, what is the proof of such a variety of the absorbent function in instances of this kind? Why should the brain accommodate itself to pressure in one case and not in another? And, in what way can the *place* of the extravasation influence the result, seeing that in both the cases we are supposing the pressure was productive of the cessation of the offices of the nerves?

I would ask also, what is the seat of chronic paralysis? We have seen in our physiology that sensation and motion are dependent both upon the condition of the nerves, and that of their centres. Is it that the nerves assume a state of inaptitude for the renewal of their function, while the process of reparation is going on in their centre; or is it that the function of their centre is permanently prevented by the original injury produced by extravasated blood? It is

said that a nerve which has been long paralysed from injury of its centre, becomes lessened in size. If this be the case, I do not see how it justifies any inference of the locality of paralysis. The wasting of the nerve might be the *effect* of a suspended or an impaired function of its origin in the brain: or the wasting of the nerve may be a part of progressive disease which commences after the destruction of its office by the influence of some associated cause. It may be answered by one who thinks to settle this question promptly, do not applications made to the limbs of paralytics, such as friction, liniments with sulphuric acid, titillation, Bath waters, pumping, electricity, &c. sometimes restore their use? I would ask in turn, and have not the extremities of nerves a relation with their origins by which the latter may be affected through the medium of the former? How else do some inoculated poisons destroy, and that in some instances with astonishing celerity, the functions of a nervous centre? And, how is it that the energies of the whole nervous system are called into action by the prick of a pin, by a few drops of boiling water accidentally falling upon the foot, &c.? Another might think to settle the question with equal readiness, by saying, I remember a case of paralysis of the retinæ, in which, though there was not much fluid perceived on dissection, there was certainly a

peculiar appearance about the optic thalami. And what would the same person say provided he should meet with another case of gutta serena, in which there was no fluid, and in which also this favourite *peculiar appearance* was wanting?

These then, I say, are questions, for the settlement of which an accumulation of facts is still desirable; and the subject demands *many facts*; for the circle is an extensive one, and a solitary connection will not justify a general conclusion, which is too often made upon solitary examples; and by none more frequently, than by those who profess to hold theory in contempt, but who in fact practice more upon theory than any others, and in general upon that of the very worst kind, namely, that founded upon the loosest analogies; upon that, made with no care, with no trouble; upon that, which they do not know to be theory; which they would be offended to hear called theory: unaware of the processes of their own minds, they fancy that inferences are visible things; they are perpetually meeting with solitary facts, and as often generalizing them; until they meet with opposite facts, and it is highly probable that they would generalize them too; or else they are at their *ne plus ultra*, and conclude it is all a dark business.

The following abstract of principles may be gathered as well from the preceding pages as

from some other well known sources; there are some among them which have not here been satisfactorily verified; and there are others which have at no time been glanced at, but which I hope at a future period to support either by an increase of facts, or by a different arrangement of those we already possess.

1. The characteristic property of life is that which maintains the cohesion of parts, which (this property being absent) would run into decomposition.

2. Death is the absence of this property of life, by which the preparatory changes, and the final state of decomposition of the solid textures, are permitted.

3. The apparently most simple state of life, or that which displays the fewest properties, is the state of it in vegetables: vegetable life may be said to be the first form of it.

The second, animal life: the characteristics of which are the above-mentioned common property of life, to which are added the faculties of sensation and motion.

The third, the life of man: comprising both the vegetable and the animal life, to which

intellectual existence is superadded; it is this last which distinguishes man from brutes; it is this which gives him his high place in creation, which makes him feel the creature of another sphere, as one too mighty for his present tenement, as one who looks with regret and contempt upon the gross ties which bind him to a subordinate world; and yet how intimately connected with, how dependent upon, such transient materials! This we shall see in the sequel.

Such are the *characteristics* of the three states of life; but each characteristic is associated with other properties, which differ in the several examples. These three states, as they are comprised in man, are designated the organic, the animal, and the intellectual existence.

4. As these properties are not visible, they may be denominated spiritual, in contra-distinction to that visible matter with which they are connected.

5. This spiritual combination is primordial. It precedes the organization of the ovum.

6. The connection between life and matter, in the processes of growth, is one of affinity.

7. The connecting link between life, and the properties common to *all matter*, is found in general, though not always, in the *chemical* properties, which are superadded variously to those which are common to all matter.

8. The organic life, by an act of the affinity above mentioned, selects, governs, and arranges the solid particles of matter; securing their conformity, and fitting at once their place to their nature, and their nature and place to their office.

9. The organic life is related with the animal and intellectual properties, and is affected and modified by them, and its acts of affinity are also modified by them: it is by this relation that mind influences matter, in some unaccountable phenomena, but which notwithstanding they have been thought unaccountable, remain obstinate facts; they are expressed in the instances of navi materni, &c.

10. Each department of the spirit is vastly, infinitely, compounded; and it displays a new or a modified state in every different act: in bone, it elects and arranges osseous matter; in muscle, muscular substance; in brain, cerebral substance, &c.: its modes of change are by decomposition or partial analysis, and by new

alliances with related properties, before denominated by privation, and by communication of influence.

11. The organic spirit exists, and acts, when both the animal and intellectual existences have ceased; or, rather, have assumed another form.

12. The animal and intellectual existence, in its present form, never survives the organic; but they are both (that is, their properties) distinct from the organic spirit, and existed before it.

13. The influence of the properties of the three existences are often mixed, and reciprocal; the instances of which can be expressed only in the detail.

14. The organic spirit is allied with all the parts of texture: the animal and the intellectual properties are allied only with certain parts, and they modify the organic spirit in the formation and development of such parts; and under that change which makes their characteristic properties exclusively cease to be conspicuous, they still concur with the organic spirit to preserve, as they did to form, the textures with which they are allied.

15. The order of relation between the parts of a human being is, the organic spirit with the animal; the animal with the intellectual; all three with the chemical adjuncts of matter, and the chemical with the common properties of matter. This order of relation is observed, and reversed, perpetually; so that the first may be affected by the last, and the last by the first; and the examples are those where such reactions may be traced, and they are both familiar and numerous.

16. The precise relations between these properties are determined by a remoter set of causes, which are latent.

17. Some particular relations of the nervous system have been spoken of: it has been hinted that the nerves are the seat of the organic life; but this spirit is not defined, it exists every where: its growth has preceded that of the textures; their development has been conformable with its development. It is the cause of primitive growth, and both precedes and governs the processes of regeneration. There are relations of affinity and reactions between it in its different seats, and the nerves are the media of this reciprocal influence, or the organic spirit allied with the nerves is interested in them all.

18. The organic spirit is identified in its several seats, both by the vital properties peculiar to one seat, together with those which are communicated to it from another.

19. The relations of one part of the spirit with another are, first, with respect to the influence received, which determines, or helps to determine, its identity in one sphere; and, second, with respect to the parts to which in turn it communicates an influence. Both these relations are direct or mediate: direct, as when the function of one part ceases from the prevention of intercourse with one other; mediate, as when the function of one part of the spirit is made to cease by the destruction of another on which it is dependent, which last has been previously changed by an influence affecting a third, which is not related with the first, except through the medium of the second.

20. Some parts of the entire spirit appear to be independent of others; that is, they are not changed by a cessation of intercourse; others, as above remarked, exercise a function which results from conjoined properties. Thus a nerve possesses, as has been demonstrated, some properties of its own, which are superadded to its organic life, but which require to be modified by the intercourse of properties with the brain,

in order to identify *animal life*, which is capable of sense and productive of motion.

21. There is no part of the spirit, or no seat or sphere of it, which is not related in the way of reagency with some others; but the spirit presiding in some spheres, is of a more general relation than that which exists in others. Again, it is the business of physiology to find out these relations.

22. The spirit is perpetually transfused, and passing into, and mixing with other substances; and in this process of transfusion its form is changed: this is a tendency of the spirit which acts perpetually; but its identity is renewed from a source, principally in all its parts, and wholly in some, in the way of assimilation before mentioned.

23. The parts of the spirit are related mediately with each other, not only through the intervention of other parts of the spirit, but also through that of its material connections; the most important example of which is that through the medium of the blood: thus it maintains itself by direct and by preparatory processes. The organic spirit, or principle of life, residing in the structure of the stomach, performs one act of preparation on the food, termed its digestion;

that residing in the liver, another, which is done by an intermediate relation, namely, by an act of the living principle in the liver on the blood, forming bile, which is mixed with digested food: the principle allied with the intestines then performs other preparatory changes on the food; that with the lacteals another; and that residing either in the heart, lungs, or diffused vascular system (for it is not determined which), another, by which food is made blood; which product is again related with the spirit in all its seats. Whatever share the chemical or other properties may have in these processes, is determined by the living principle; *for this is antecedent to the aggregation of matter, and to the combination of chemical properties.*

24. The spirit is affected by external agents; and spontaneously, by latent properties: they act by preparatory and subordinate changes, and might be the gradual accessions which the spirit has acquired in the lapse of ages, and first introduced by external relations.

25. The changes of the spirit are those of its properties. Health is constituted by definite properties, disease by deviation from such definite properties, which deviation is still compatible with the characteristic acts of a principle of life: death is such a deviation of properties, that

life is no longer recognized by any characteristic act.

26. The origin of spontaneous disease is in the spirit: its material and chemical alliances may become reagents.

27. The disease of the spirit in one seat will influence the spirit of connected parts, according to a relation which is determined by another set of causes different from those which perform the actions characteristic of life.

28. As disease is a digression from a defined identity, so the principle of therapeutics is to restore the defined identity, which constitutes health: but the properties concerned in the deviation are never known; and if they were, it is probable that we should not know where to look for those which would perform the work of restoration. On this account, the practice of medicine is founded on no direct principle.

29. We observe that the tendency of a first deviation from health is to proceed with successive changes, which influence related parts; and the series is either terminated in death, or else by a return to the primitive state of health. These results will depend upon latent causes.

30. The latter, namely, the tendency of the living principle in any of its seats to return after a certain digression to its original state, has been called a vis medicatrix; and it is upon this vis medicatrix that the practice of medicine is founded in the following way.

31. Disease is the affection of some organ (or essentially the principle allied with it): this affection (a process of causation having commenced) will proceed through a series; if another affection be produced, as by medicines, in the same part, the series of causation will be changed; and we trust to that vis medicatrix, or, as I should say, to a set of latent causes, to determine the latter series towards the restoration of health. Sometimes we are disappointed; and, for want of the direct principle before mentioned, we may accelerate the termination in death. This is the basis of the expectation of cure by a mode which was a favourite one with Mr. Hunter, namely, by exciting an artificial, in order to cure a natural disease; or by making one disease *supersede* another, an expression which is founded on a still grosser conception of an analogy. Our specifics seem to operate upon the direct principle; but this is a deception; for one man may be cured of syphilis by mercury, and another may be killed by the remedy: that is, a latent cause, the tendency of which is to consumption,

may be called into action; and he would die sooner from the series of changes produced by the remedy, than from those occasioned by the disease; and the same observation holds with respect to the other specifics.

32. The practice of medicine entertains no better design than this, namely, *to affect the condition of disease;* and we trust to latent causes for the event. We are not to select *any* remedies with this view; we must be guided by that which we dignify with the name of our *experience:* it is however upon this principle that all sorts of treatment of the same diseases which have prevailed in different ages, have succeeded; the state comprising disease has been reaffected by the practice, and a change in the spontaneous processes induced, which change, as it is a propensity of the spirit to preserve the state of health, is very frequently favourable.

33. Our clearest practice is that which best suits our faculties (which are rather accommodated to gross perceptions than to refined views), namely, that, when we undertake to remedy the *effects* of original disease; that, when we design to look no further than to influence the reagents; that, when we do not aspire to know, or to influence, the original cause, except in treating the effects.

I have given this brief sketch, in order to exhibit some principles which have been kept in view in the preceding pages: they are however but a faint and limited abridgment of the propositions which are elsewhere made in detail, and connected with the evidence which affords them support. They are here propositions abstracted from facts: I trust however to show that these form a very inconsiderable part of the propositions which belong to the subject; and that none of those which are designed as propositions, stand upon a weaker evidence than that which is ordinarily received. If, however, there be any truth in this limited view, we shall be enabled to guess at some real impediments which have opposed the progress of the healing art: we shall also think that these impediments are still insurmountable; that the healing art must remain defective, because the knowledge of it would require the possession of faculties which have never yet been granted to man.

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